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<i>Experiments in the Social Sciences: PROFESSOR HAROLD F. CLARK</i>	393
<i>Translocation of Carbohydrates in Maize: DR. W. E. LOOMIS</i>	398
<i>Obituary: Recent Deaths</i>	400
<i>Scientific Events: The Soviet Academy of Medicine; Destruction of the Bureau of Science at Manila; The Nutrition Foundation; The Pennsylvania Academy of Science</i>	401
<i>Scientific Notes and News</i>	402
<i>Discussion: "Facts" and "Interpretations" Regarding Race Differences: DR. HENRY E. GARRETT. Marine Fouling and its Prevention: DR. MAURICE COPI-SAROW. Encystment and Excystment in Chaos: DR. EDWARD J. WENSTRUP. An Inorganic "Bouncing Putty": D. L. HANNA</i>	404
<i>Scientific Books: The Physics of Music: PROFESSOR ABE PEPINSKY. Psychopathology: DR. HARRY C. SOLOMON</i>	408
<i>Reports: Over-the-Counter Sale of Sulfonamide Drugs: DR. GEORGE MILLER MACKEE, DR. MCKEEN CATTELL, DR. RUSSELL L. CECIL, DR. ROBERT A. COOKE and DR. E. H. L. CORWIN</i>	409
<i>Special Articles: The Fate of Estrogenic Metahormones in the Liver: PROFESSOR ALEXANDER LIPSCHÜTZ, DR. CARLOS BECKER, DR. RAUL F. MELLO and DR. ANDRÉS RIESCO. The First Stage of Antigen-Antibody Reaction in Infectious Mononucleosis: DR. PHILIP LEVINE and ELEANOR L. GILMORE.</i>	

Alterations in the Antidromic Potential of Motor Neurons Following Chromatolysis: DR. BERRY CAMPBELL. The Antibiotic Activity of Extracts of Ranunculaceae: DR. BEATRICE CARRIER SEEGAL and DR. MARGARET HOLDEN. Distribution of Radioactive Sulfur in the Rat: DR. H. O. SINGHER and DR. L. MARINELLI. Experimental Verrucous Endocarditis: DR. WARD J. MAC NEAL, ANNE BLEVINS, ALICE E. SLAVKIN and HELEN SCANLAN 410

Scientific Apparatus and Laboratory Methods: Grapevine Injection Apparatus: DR. W. O. WILLIAMS. A New Herbicide: DR. A. S. CRAFTS 416
Science News 12

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EXPERIMENTS IN THE SOCIAL SCIENCES¹

By PROFESSOR HAROLD F. CLARK

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CAN you experiment in the social sciences? With the help of the Sloan Foundation and several universities, I have had the chance to try to get a partial answer to the question. What have we learned from these efforts regarding the limits of experimentation in the social sciences? Does experimentation offer any promise of throwing light on complicated social problems? Before we turn to our efforts at experimenting let us briefly review something of the history of science and see if we can obtain any light on the problem.

Animals hibernate in the mud at the bottom of streams in the winter. That is obvious; the animal

¹Address of the vice-president and chairman of Section Q—Education, American Association for the Advancement of Science, Cleveland, Ohio, September 12, 1944.

is here in the fall, he is here in the spring. He must have gone in the mud. At least some of the greatest minds in the world said so only a little over two thousand years ago.

Every one knows that the way to cure a person of witches curse is to rub his head with ground-up bodies of dried wasps. So, at least, said some of the great medical authorities of the middle ages.

Rosen, in his recent book on "Hollywood," says many of the famous actors and actresses believe in the magic of numbers. One particular actor would never go on the stage unless the number of steps was exactly nine. He knew he would hurt himself if it was any other number. One day the studio built a set with ten steps on it. The actor insisted the steps be changed to nine. They were. That day he fell down

the steps and broke his arm. He said, "See if they had not changed those steps, I would have broken my neck."

The progress of the extension of scientific attitudes is slow and difficult. Watching its extension is one of the most interesting of all intellectual activities. The people of any age in the past have been willing to adopt scientific methods and use science in helping solve only a small fraction of their problems.

There are many ways to indicate this slow expansion of the scientific attitude. One is to look at the great scientific organizations and see the expansion of topics covered in their programs and articles. The Royal Society of London is one of the oldest and greatest scientific organizations in the world. What has it discussed during this long history?

There was little or no attention to social problems or to the social effects of science in the early centuries of the organization. I have not read all the articles in the publications of the society. The index, however, shows that few if any articles were dealing with social problems in the early years. I went through the volumes with care, and if there was any indication from the index that an article had social implications, I turned to the article itself. What range of topics did I find discussed?

When the Royal Society was established, its conception of science was pretty largely limited to the physical world. In the general index to the Philosophical Transactions of the Royal Society, volumes 1 to 70, we find no reference to what we could call social problems, as such, and certainly nothing remotely resembling experimental procedure in any of the social fields. In fact, the index is particularly interesting because it indicates that social problems were not considered as being subject to scientific procedure. The words in the index just before and just after the place "social" would appear are of more than passing interest. The words appear in the index in this order:

soak

Social war

[just to ease your minds, however, "social war" does not refer to warfare among social classes, but the article to which it refers has the following interesting title:

"Elucidation of an Etruscan Coin Emitted from the Mint There about the Time of the Social War."]

soil

Solanum Lethale

So, we can see clearly there is not reference to social problems to society or to any similar words.

If we turn to the heading "Man," we do not find much more to indicate an interest in social problems. There are several articles appearing under the heading "Man." The first article has this odd title, "Account of a Man of a Strangely Interesting Na-

ture." The next article is even more astounding. "The Anatomy of a Decepid Old Man of 109 Years." You may use your own skepticism regarding the next title, but it actually appears as a paper in the Transactions of the Royal Society, "Letter Concerning a Man Who Lived 18 Years on Water." The next entry under the heading "Man" is "Extract of a Letter Concerning Two Men of an Extraordinary Bulk and Weight, An Essay Toward Ascertaining Specific Gravity of Living Man."

These are papers that appeared during the period of seventy years in the Transactions of the Royal Society concerning man. Any other headings that might indicate a social orientation of articles produce much the same result. I think it is safe to say that the Royal Society in the first seventy years of its existence was so absorbed in trying to extend science in the physical fields that it had little time to deal with social problems. This is no criticism of the Royal Society and its distinguished members, but simply is another indication of the slowness with which the scientific approach extends into new fields.

If we take the period from 1781 to 1820, our results will be much the same. The index of the Philosophical Transactions of the Royal Society covering this period of approximately a generation gives no word at all where "social" would appear in the index. First the word "Sobieski" appears; "social" would appear next, but there is no entry under this heading. The next word in the index is "soda," the next "sodium." Such words as "society," "humanity," "man" failed to produce any articles that could remotely be called social in their application.

The volume of the index covering the period 1821-1830 lists the word "snipe," the next word is "soda." "Social" does not appear at all. I think it is safe to say that the papers reported in the Transactions of the Royal Society in the early part of the last century had not yet reached the stage of dealing with what we would call the major social problems. Science was still engaged in expanding its field in the physical world, and again this is no criticism. It is difficult to see how any other alternative would have been possible at the time.

The curious thing is that as a hundred years are passed over, and we take the early volumes of this century, exactly the same situation will be found to exist. In the Royal Society Catalogue of Scientific Papers Index to the "Proceedings of the Royal Society of London, 1905-1930" there is no reference to social or society. The index contains the word "snow" and the next word is "Soddy." A study of such headings as "man," "humanity" and similar words also indicates that papers dealing with what we would call the social sciences were not included.

Of course, I am not implying that long before this there had not been the rise of other organizations in England that were taking a scientific approach to the study of human and social problems. But it is of more than passing interest that seemingly the Royal Society paid little attention to such problems even in as recent a period as 1901-1930.

In the 1930's, we find scientific organizations all over the world taking a far more active position toward extending the scientific attitude into social and economic fields. This was due, in some part at least, to the attacks that were made on science, by some inaccurately informed individuals. But even more, I think it indicates further extension in the slow process of expanding scientific techniques into additional fields. This may turn out to be the most helpful and important thing that has happened in our day and generation. Special societies were organized early in the nineteenth century which tried to get a more scientific approach to human and social problems.

Seemingly, many of the great and powerful scientific organizations were slow to expand their activities to include the social field. By the end of the first third of the twentieth century, it was crystal clear that such expansion must take place. This extension is necessary both for the safety of society and the further growth and expansion of science itself. In the 1930's, we have both the great British and American scientific associations going on record officially as saying that organizations of scientists must devote systematic attention to the social problems around us. The scientist knows better than any one else that only the systematic and careful study of social problems can possibly provide a dependable answer as to the method of solving such problems.

The rest of this paper will be devoted largely to an effort to try to indicate some of the methods by which science can expand into the social fields. One illustration will be discussed at some length to show that there are even much wider ranges of experimental possibilities than have been seen by most people in the past.

Should scientific techniques be extended to the social field? Many will say it should not be tried. Others will say it can't be done. Still others will say that the problems are so difficult that it is impossible to isolate individual factors and find their influence.

It is always difficult to move scientific investigation into new fields. If it were not difficult, the Greeks, the Romans, the people in the Middle Ages or even those living in Hollywood would have done something about it.

Some birds and animals are here in the fall, gone in the winter, here in the spring. Some of the greatest minds of Greece could not figure out what had happened.

People do not like to reach the wrong answer on this or any other problem just for the sake of having them wrong. If they get the wrong answer it is because they do not know how to get the right one. After the problem is solved, it always looks easy. Electric lights look very simple to us, but more than one man was called crazy when trying to make them.

For thousands of years the extension of the scientific attitude into new areas has gone on. Prehistoric man was not sure what happened when seeds were planted. A few seeds went into the ground; many seeds appeared in the plants. The gods had intervened in human affairs for man's benefit. Primitive man did not understand what had happened. He gave some kind of magic credit for the result and let it go. Some of our ancestors argued it was magic and insisted that the gods would be offended if investigations were made. Some argued that it was wise to investigate and see what made the seeds grow. It was only after many centuries that the investigators won and the grain and crops were improved.

A recent "History of Bread" tells us that for long centuries in Egypt the making of bread was a mysterious art. The sacred dough to make bread rise was kept as carefully as the sacred fires. It was many generations before man dared investigate the cause of the bread rising, but finally he did. Better and cheaper bread resulted.

After many thousands of years, it became respectable to study scientifically the causes of the sprouting of grain, the growth of plants, the life cycle of animals. Much of the physical world could be investigated directly. For some thousands of years then man has been willing to investigate certain parts of the biological world. In the nineteenth and twentieth centuries, man has been willing to investigate scientifically a small part of the social and economic world.

We are speaking of science as opposed to magic, superstition and untested ways of doing things. Science is not opposed to common sense, but it is an effort to check results of common sense in every way possible. The order of investigation might be as follows: the results of common sense, careful observation, then controlled experimentation.

Obviously, this is not always the sequence of events. In astronomy, for instance, experimentation is severely limited. No one has yet been able to make a solar system to match and serve as a control to the observed one, and yet astronomy has been able to produce work of the greatest scientific importance.

Experiments are a great help if you can get them. If it is impossible to experiment, there are many other ingenious ways to proceed to get more scientific results than man has previously had. How then would we expect to broaden scientific technique into the social and human fields?

The following illustration may throw some light on the problem; Because of observed results in certain communities in Texas, many dentists and doctors have been led to believe that a small amount of fluorine in the water will retard or possibly even stop the decay of teeth. Dentists might argue about this for generations. The obvious thing to do is to attempt to get experimental evidence but how can you experiment upon a community? Seemingly, it is not too difficult once the decision is made to try it. The experiment has been set up and is being tried over a period of ten years in two cities in New York State. Newburgh and Kingston, two small cities of about the same population, have been chosen. These two cities of approximately the same size and population have much the same social and economic pattern, and both are in the Hudson River Valley and presumably would have the same general factors affecting the teeth of the populations.

In one city fluorine is to be put in the water. In the other it will not be put in. Will the teeth be better in the community that has the fluorine in the water? This seems like the only sensible way to try to get an answer to the problem—set up an experiment and see what happens.

It is quite true that there are literally thousands of unknown factors that work in both communities. The scientist accustomed to working in the physical laboratory with what he called a controlled experiment would be appalled at the uncontrolled factors in this situation. No one would maintain that this is a perfectly constructed experiment. No one would maintain that it would finally answer the question. If the experiments are repeated in a dozen other communities with a dozen other controls in various sections of the country, and the results always turn out the same, it would probably shed a great deal of light upon the problem. There are an enormous number of social and economic problems that could be experimented upon in exactly the same way. Experimental and controlled communities could be chosen and the experiment carried on.

Does the study of mathematics discipline the mind of the person that studies it? Some say "yes" and some say "no." Some of the world's greatest mathematicians who are most careful in their own field, jump to conclusions when they discuss such a problem as this. They do not use the scientific method. Isn't the sensible way to answer the problem to set up an experiment and try to find out? This, of course, has been done. You may not be able to control all the factors, but if wise men carry on the experiment, men who use the best common sense and the most careful observation, you will get a better answer than will equally wise men with only common-sense plus observation.

In the social fields there are so many variables that you can not be sure of causal factors. Is that not reason for more experiments rather than trying to guess? I would like to illustrate by one problem how it is possible to set up an experiment to try to throw some light on an important social and economic problem. I am going to discuss the illustration at length because I believe it is possible to extend the technique to help solve thousands of our most pressing social and economic problems.

Here is a low-income, rural community. The diet is inadequate. Is the poor diet due to the fact that the people are too shiftless to make an effort to improve the diet or is it due to the fact that they do not know what to do? I can find you plenty of learned discussion in the best tradition of the witch doctor arguing that the people are lazy. I can find other equally good arguments maintaining that the problem is ignorance. Is the wise thing to go on discussing the problem forever, or can we get some experimental evidence?

Much of the land in this community is hilly. The farms are badly eroded. Much of the top soil had washed away. As soon as the people were shown how to improve these conditions they made changes. In one community, entirely agricultural, almost no poultry was raised although it was a very good place for raising it. Would the people raise chickens if they were given adequate instructions? The State Experiment Station bulletins and county agent were there to give them the information, so the answer must be that lack of information was not the reason. But the bulletins were too difficult to read and the county agent went only where he was asked. Could simpler material than the bulletin be prepared and would it be used? The only way to get the correct answer was to try and see.

A series of bulletins at the first-, second- and third-grade levels were prepared on gardens. Improvement and expansion occurred in many gardens. Some one will say that other factors were at work during this period trying to improve gardens. How do you know that your school instruction and readers made the change? The obvious way to answer the problem is to set up controlled experiments. This is difficult but no reason for not doing it. That is exactly what we did. We found one experimental community. Then we looked for a control community as nearly like it as possible. We wanted two communities of the same general economic status, same educational level, same racial stock, same age distribution, same religious and cultural beliefs. In other words, we wanted two communities as nearly alike as possible.

If the diet improves in the experimental community the new education program might have caused

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change. On the other hand, the diet might have improved in the control communities. Many factors might have been working on a state or nation-wide basis. If the diet improved more in the experimental community than in the control community, presumably our education material caused the difference.

Chance factors may still be the real cause. To answer this objection, we took a second experimental and second control community. If the second experimental community improved more than the second control community then the evidence is very strong. Maybe there were local factors working here that would not be at work in the other sections of the country. The way to answer this problem was to take another set of control communities and another set of experimental communities in a different section of the country.

Perhaps the schools could improve diet. Could we improve any other important area of living? The way to find out whether schools could improve another area was to try it. For this additional part of the experiment the problem of housing was chosen. If we teach people in low-income communities how to improve their housing, will housing improve? That has been debated for centuries and the debate may go on for other centuries. The only sensible thing to do was to try it and see what happened. Steps were repaired, screens were fixed, gardens planted with trees and flowers, houses were painted, chimneys repaired, holes in roofs were stopped. The evidence seems impressive that if you show people how to improve their homes, they will do so.

Experimental communities changed. Control communities did not change as much. The evidence is strong that the education caused the change.

If the housing information were made available in the second community what would happen. Would housing conditions improve? If the information could improve conditions only in one community, the effort would hardly be worthwhile from the standpoint of the Sloan Foundation or society at large. Could the same results happen in a second and third community? Try it and see. It is the only way to be sure. It was tried in a second and third set of communities, experimental and control.

If you took the problem of clothing and tried the experiment in another section of the country, would conditions in low-income communities improve if they knew what to do? The scientific answer would be to try it and see. A conservative New England state was chosen. The evidence seems to be that the people will make a long series of improvements if they know what to do.

In regard to food, clothing and housing in three different sections of the country in various low-income communities, if people are told what to do to

improve conditions they are ready and willing to make a wide variety of improvements. One hesitates to generalize too much on the evidence. But within the limits of this evidence, and it is rather broad, it does seem that low-income communities can and will improve themselves if they are shown how. What are the limits? Only further experimentation can tell.

Such experiments are expensive. That may be one reason so few of them have been carried on. This series of experiments have cost the Sloan Foundation a very large amount of money. But in terms of the hundred of millions and even billions that have been spent and the other billions that will be spent to aid low-income communities, the amount is trivial. Eighty or 90 per cent. of all the population of the world live in communities of lower economic standards than our experimental communities. The United States in the next five years will doubtless extend many billions of dollars of relief to various sections of the world. I would be the last to object to this. Many persons believe that the way we will extend the relief will leave the communities in essentially the same conditions as they were. A tiny fraction of this amount if spent in helping the people to find out far more adequate ways to help themselves seemingly would produce profound and permanent changes.

In the normal course of human developments there probably will be future occasions in this country when extremely large sums will be extended for relief. Seemingly great results could be attained if the people were really shown how to help themselves. This help, of course, has to be in terms of their own environment—things they can do and things that are within their experience.

The immediate practical results of the experiments seem to be important. They may be of far greater importance in indicating more clearly than has formerly been done the desirability of extending the scientific method into other fields of human activity. It seems reasonable to say that large-scale experimentation is possible in the social sciences and that such experiments should be greatly expanded.

I will refer very briefly to other types of experiments that could be carried on. Some people advocate one method of setting up a health program in a community. Other people advocate another method of setting up a health program. Why not set up both types of programs in comparable communities and attempt to show results in terms of actual community change? Any number of suggestions are being made to deal with the problem of juvenile delinquency. Why not try out the various methods and see which of them produces the best results? Various schemes of organizing school boards of collecting local taxes are advocated. Try out the various methods, keeping an accurate record of the results and make your de-

cisions on the basis of the facts. County school boards can be set up on various bases, which will give the best results.

Some years ago I made the suggestion to the governor of one of our progressive states that every county in the state should be carrying on some kind of carefully controlled experiments to answer the problems of public administration, governmental organization, public health and education of that state. If the proper kind of arrangements were made, this could be done with relatively small expense. The results should be enormous in terms of human welfare. Every community in the state would know that it was a part of some broad social experiment attempting to give the answer to public problems. A great deal of volunteer effort of young people from school and other groups could be obtained. The time might easily come when a community would pride itself

more on the type of scientific experiments in the social field which are carried on than it would in size or wealth.

It is quite true that we can not experiment in the social sciences in the narrow and rigid way that a laboratory scientist does, but we can experiment usefully. The various parts of the Sloan experiment have thrown great light upon the problem of effect of school instruction on changing food, clothing or housing in a low-income community. If the experiment is repeated in a great variety of forms and places, we should be able in a few generations to build up a far more accurate picture than any one now has of the effect of school instruction in changing a community. The experimental results available should make us very optimistic of the possibilities of extending scientific procedures into social problems. We can experiment in the social sciences with great profit to mankind.

TRANSLOCATION OF CARBOHYDRATES IN MAIZE¹

By Dr. W. E. LOOMIS

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It has been the aim of a large group of plant physiologists, with whom the writer likes to identify himself, to reduce the reactions and responses of living plants to processes demonstrable in our laboratory test-tubes. The evaporation of water from leaves, the absorption of water by roots and the diffusion of carbon dioxide through stomates are processes which we think we can soon, if not now, explain in terms of thermodynamics and the calculus. The tremendous strides of the last decade lead us to hope that we will be able eventually to explain photo-periodism, respiration and photosynthesis in terms of specific physico-chemical reactions. I think that we can see glimmerings of a similar basis for such vitalistic processes as protoplasm synthesis and the physiology of inheritance, but I confess myself baffled by some of the phenomena of translocation in plants.

The problem of the translocation of organic compounds in plants can be divided into the sub-problems of (a) the tissues concerned, (b) the compounds moved, (c) the mechanisms involved and (d) the controlling factors.

THE TISSUES CONCERNED

The experiments of Curtis² and of Mason³ are

¹ Address of the retiring president (1943) of the American Society of Plant Physiologists, presented at the Cleveland meeting of the American Association for the Advancement of Science, Cleveland, Ohio, September, 1944.

² O. F. Curtis, "The Translocation of Solutes in Plants." New York. 1935.

³ T. G. Mason and others. *Ann. Bot.*, 42: 189-253, 1928, and subsequent volumes.

generally considered to have established the phloem as the primary tissue of translocation. The maize plant with its many closed bundles is not adapted to studies of tissue function by ringing, etc., but we must assume that here, as in sumac, privet and cotton, the phloem is the important channel of food movement. The phloem of the stem, however, is surrounded by parenchyma and can not be studied separately. Our first task, therefore, has been to show that sugars are transferred between the phloem and the pith parenchyma, and that an analysis of the entire pith is capable of furnishing information on the progress of translocation. Such evidence has been obtained by covering plants growing in the field with light-tight covers and analyzing their tissues at intervals.

Typical data from one of these experiments are shown in Table 1. Ear shoot development was pro-

TABLE 1
CHANGES IN SUCROSE CONTENT OF TISSUES OF MAIZE PLANTS
HELD IN DARKNESS

Tissue and stage	Sucrose, percentage of green weight			Loss
	At start	After 24 hours	After 72 hours	
Leaves				
Tasselling	2.42	0.11	0.11	2.31
Milk	2.65	0.20	0.13	2.52
Milk-bagged ..	2.30	0.46	0.76	1.54
Stalks				
Tasselling	0.59	0.22	0.26	0.33
Milk	8.54	6.20	5.03	3.51
Milk-bagged ..	8.01	7.38	7.19	0.82
Shanks				
Milk	5.42	4.25	3.07	2.35
Milk-bagged ..	3.88	3.70	3.10	0.78
Cobs				
Milk	2.32	2.19	2.39	-0.06
Milk-bagged ..	2.36	1.55	1.34	1.02

ing rapidly at the tasseling stage but no grain was present. The milk stage was chosen, on the basis of previous experiments, to represent maximum translocation to the developing grain. A third series of plants was bagged to prevent pollination and grain development, and was then sampled on the same dates as the milk-stage plants. Changes in the sucrose content of the pithy stems and shanks were as great and almost as fast as the changes in the leaves. These changes, however, were dependent upon the presence of developing grain as a storage organ and were nearly absent in plants without grain.

At least a partial equilibrium between the sucrose content of the translocating tissues and of the much larger volume of pith is indicated. Such an equilibrium raises a morphological-physiological problem, for the two tissues are separated by the lignified cells of the bundle sheath. No parenchyma passages across the sheath, analogous to wood and phloem rays, have been observed or described. The more popular of the present theories of translocation assume that movement is normally restricted to living cells with plasmodesmal connections, and that little or no translocation would occur through tissues of the type surrounding the maize bundle.

THE COMPOUNDS MOVED

We assume that the carbohydrates translocated in the maize plant will be soluble or dispersable in the plant sap. Four compounds are available: fructose, glucose, sucrose and a part of the dextrin. The evidence here seems to be clearly in favor of sucrose. Nearly all the sugar and most of the carbohydrate of the leaf is sucrose. Sucrose losses clearly precede those of other fractions, particularly of dextrins. In the stems sucrose is low in the growing plant but accumulates rapidly after tasseling until it constitutes 6 to 10 per cent. of the green weight of the plant. Overnight losses from the stalk are nearly pure sucrose, and sucrose is the only carbohydrate which consistently shows losses in darkness along the translocation pathway of leaves → sheaths → stalks → ear shanks (Table 2).

TABLE 2

CARBOHYDRATE LOSSES FROM TISSUES OF MAIZE PLANTS HELD 24 HOURS IN DARKNESS. DATA ARE PER CENT. OF GREEN WEIGHT

Tissue	Fructose	Glucose	Sucrose	Dextrin
Leaves	0.00	0.09	2.45	0.83
Sheaths	0.03	0.19	1.50	0.44
Stalks	-0.16	0.08	2.34	0.05
Shanks	0.00	-0.04	1.17	-0.10
Cobs	-0.04	0.01	0.13	0.52
Average	-0.03	0.07	1.52	0.35

The rapid initial losses of sucrose and the slower losses of dextrin suggest that the sugar is the trans-

location form and that the polysaccharide is moved after digestion. The data of Leonard⁴ should be recalled, however. He found that carbohydrate interconversion in maize leaves was so rapid that accumulation forms were not affected by the form supplied in the transpiration stream. Leaves injected with fructose accumulated both glucose and sucrose at the same rate as those injected with glucose. Such transformations may not be so rapid in the stem, however, and the evidence available indicates that sucrose is the important carbohydrate of translocation in maize.

THE MECHANISM OF TRANSLOCATION

Two mechanisms to explain translocation have been advanced. The Curtis-Mason hypothesis of accelerated diffusion along a positive gradient of the moving substance, with acceleration by protoplasmic streaming, interfacial movement or unknown factors, and the Muench hypothesis⁵ of mass flow on an osmotic gradient. Neither of these hypotheses is prepared to explain carbohydrate translocation in maize, where not only total but all individual gradients from leaves to stalks are negative by 2 to 50 or more times. Sucrose disappears from leaves containing 0.3 per cent. and moves through tissue containing 7 to 8 per cent. of the same sugar in at least partial equilibrium with the phloem. Glucose gradients may be nearly as steep and fructose concentrations in the stalks 200 times those in the leaves are not uncommon. The difficulty might be explained by a secretory action in the leaf—if that is an explanation—if it were not for evidence that such secretory, negative gradients are not only widespread throughout the plant, but that they show an interrelated polarization. We suggest that new hypotheses on the mechanism of translocation are needed.

POLARIZED TRANSLOCATION

Leonard's work from this laboratory on the polar export of sugars from the blade mesophyll of sugar beet⁴ was preceded by still unpublished work showing the same phenomena in maize. More recent work with maize and other plants has corroborated these findings, and Phyllis and Mason⁶ have shown a similar reaction in cotton. If mature sugar beets are darkened to prevent photosynthesis, the blade sugars are moved into the petiole and root, and the leaf blades die from lack of sugar while the roots contain 15 to 20 per cent. of sucrose. Young leaves on the same plant grow vigorously on this stored sugar. Apparently some change in the maturing phloem or

⁴ O. A. Leonard, *Am. Jour. Bot.*, 25: 78-83, 1938, and 26: 475-484, 1939.

⁵ A. S. Crafts, *Plant Physiol.*, 6: 1-41, 1931.

⁶ E. Phyllis and T. G. Mason, *Ann. Bot.*, 47: 585-634, 1933.

border parenchyma of the leaf eventually stops upward translocation. At the same time sugars are almost quantitatively exported from the blade against steep concentration gradients.

The reactions in maize are similar (*cf.* Table 1). Sucrose concentrations of 2 to 3 per cent. in the blade drop to 0.2 per cent. in 24 hours of darkness, while total sugar concentrations of 3 per cent. drop to 0.3 per cent. Removing or bagging the ears to prevent translocation to the grain gives a leaf drop of from 3 to 4 per cent. to 0.5 to 1.0 per cent. total sugars, showing some backing up into the leaf.

A reversed polarization shows in the cob where sterile (bagged) cobs show a drop in sugars after the plants are darkened, but fruiting cobs, from which sugars are being rapidly moved to the grain, may show sugar gains of 30 per cent. of their original concentration after 72 hours of darkness (*cf.* Table 1). A polarized movement toward the developing grain is indicated.

A still more striking demonstration of the polarizing action of developing grain may be obtained by removing all leaves from the main axis of a plant having well-developed basal branches or suckers. If these leaves are removed just before normal pollination, the ear shoot fails to develop and the stalk is barren. If the nearby leaves are left a few days until a polarizing action of the developing embryos can be established, full-sized ears can be produced by translocation from leaves on the sucker branches 8 or 10 feet from the ear. Typical data are shown in Table 3. No appreciable grain development, in

TABLE 3

YIELDS OF MAIZE PLANTS WITH BASAL BRANCHES WHEN THE MAIN STALK WAS DEFOLIATED BEFORE AND AFTER POLLINATION

	Defoliation—days after pollination					
-2	0	1	2	4	8	Ck
Yield as a percentage of check						
10.9	34.4	48.7	59.0	73.7	99.5	100.0

terms of dry weight, had occurred in the plants defoliated eight days after pollination. Polarization had been established, however, and normal grain was developed with food obtained from the leaves of sucker stalks. Normal yields could be obtained without defoliation at the time of pollination by first bagging the ear shoots and holding the plants without defoliation until they had built up a local food reserve sufficient to initiate embryo development. All grain produced by the plants defoliated two days before pollination (-2) was on stalks held in this way. When the main stalk was defoliated just before ear shoot emerged from the sheath, all further development of the shoot was prevented.

Unpublished and general evidence indicates that polarized translocation is a factor in many correlation and inhibition reactions of plants generally, as well as of maize.

SUMMARY

(1) The pith of maize shows a changing sugar content with changing sugar movement in the phloem which would not be predicted from our present picture of the morphology of the maize bundle.

(2) By all tests of changing concentration, sucrose is the important carbohydrate of translocation in maize. Interconversion of the several sugars is rapid, however, to permit a final conclusion.

(3) An hypothesis of translocation in maize may not only account for movement against an osmotic gradient, but against gradients of each of the substances which might possibly be translocated. Secretory translocation certainly occurs between the leaf mesophyll and the phloem and probably along the phloem itself.

(4) Translocation in maize is polarized, out of the leaf, out of the xylem and toward the developing fruit. Polarized translocation out of the leaf is established during the later stages of tissue differentiation. Polarized translocation toward the fruit is established in the early phases of embryo development and does not develop in the absence of pollination.

OBITUARY

RECENT DEATHS

DR. THOMAS F. HOLGATE, professor emeritus of mathematics and dean emeritus of the School of Liberal Arts of Northwestern University, died on April 11. He was eighty-six years old.

ALFRED VICTOR DE FOREST, professor of mechanical engineering at the Massachusetts Institute of Technology and president of the Magnaflux Corporation, died on April 5 at the age of fifty-six years.

DR. RAYMOND HASKELL, consulting engineer for the Texas Oil Company, died on April 6. He was sixty-six years old.

SIR WILLIAM NAPIER SHAW, F.R.S., meteorologist and former director of the British Meteorological Office, died on March 23 at the age of ninety-one years.

DR. HANS FISCHER, professor of organic chemistry at the Technical High School at Munich, has died at the age of sixty-three years.

SCIENTIFIC EVENTS

THE SOVIET ACADEMY OF MEDICINE¹

In accordance with a decision of the Council of People's Commissars of the U. S. S. R., July 30, 1944, an Academy of Medicine will be formed in Soviet Russia by dividing into a number of independent research institutes the existing All-Union Institute of Experimental Medicine.

The academy will consist of twenty-five research institutes in all. It will include three departments: of medicobiologic sciences, of clinical medicine and of microbiology, epidemiology and hygiene. The department of medicobiologic sciences will be entirely made up of institutes formed from VIEM, the name by which the Maxim Gorky All-Union Institute of Experimental Medicine is known and which was organized by special decision of the Soviet Government October 15, 1932, to replace the old Institute of Experimental Medicine, the only large research institute in Russia until 1918.

Thus the Institute of Experimental Biology will be formed from Dr. A. G. Gurvich's department of general biology and the biophysical laboratory, the Institute of Morphology from the morphologic laboratories of VIEM and the Institute of General and Experimental Pathology from Dr. Alexei D. Speransky's department of general pathology and a number of pathologic laboratories. The Institute of Biologic and Medical Chemistry will consist of the department of medical chemistry under Dr. Parnas, together with a number of biochemical and organic chemistry laboratories and the Institute of Physiology of the numerous physiologic laboratories of VIEM. The Leningrad branch of VIEM will be reincorporated as the Institute of Experimental Medicine and thus continue the traditions of the old Institute of Experimental Medicine. Finally, the aforementioned department of medicobiologic sciences will also include the Institute of Evolutionary Physiology of the higher nervous activity (which was separated from VIEM in 1938) and the Institute of Pharmacology, Toxicology and Chemotherapy, part of which will be made up from the respective laboratories of the institute.

The academy's department of clinical medicine will include the Institute of Neurology, which will be made up of the VIEM clinic of nervous diseases and the Institute of Experimental and Clinical Surgery, which will be formed from a number of surgical clinics, among them the clinic directed by Dr. A. V. Vishnevsky. The academy's department of microbiology, epidemiology and hygiene will include the Institute of Virusology, to be formed from the present department headed by Dr. Anatol A. Smorodintsev.

¹ From the *Journal of the American Medical Association*.

A number of VIEM's microbiologic laboratories will go to make up the Institute of Bacteriology, Epidemiology and Infectious Diseases, which is to be organized under the same department.

According to an article by Dr. N. I. Propper-Grashchenkov in the *American Review of Soviet Medicine*, December, 1944, page 108, the Sukhumi branch of VIEM will remain the same broad biologic base for the institutes of the Academy of Medicine and will therefore be part of the academy.

DESTRUCTION OF THE BUREAU OF SCIENCE AT MANILA

From information recently received from Manila I regret to report the total destruction of the Bureau of Science building, housing as it did the central scientific library of the Philippine Government, as well as the exceedingly valuable and irreplaceable natural history collections. The herbarium contained many thousands of types and a very impressive series of isotypes from the older historical collections from India, China, Malaysia, tropical Africa and Australia. In the zoological field there were very extensive collections of insects, reptiles, birds, fishes and mammals; some of these collections were also rich in types. The library, one of the largest and most carefully selected accumulations of scientific books in Asia and Malaysia, was a remarkable assemblage of basic literature. These great collections of literature and natural history specimens of the Bureau of Science represent the results of consistent efforts of a large number of individuals over a period of more than four decades. The loss is an irreplaceable one.

In the same general area important buildings that are totally destroyed or very badly damaged include the School of Medicine, the School of Hygiene and Public Health, the entire plant of the University of the Philippines, the Philippine General Hospital, the Weather Bureau and the Philippine National Library. These buildings, for the most part of modern reinforced concrete construction, were especially designed for their specific purposes. My informant states that in short the Japanese obliterated everything of scientific value, the loss of the important libraries being particularly serious. It goes without saying that involved in this destruction was all the reserve stock of the *Philippine Journal of Science* and other publications of the Bureau of Science. While basic reference works may be replaced by photostat or microfilm reproductions it will be quite impossible to secure copies of many badly needed reference works because of their great scarcity.—E. D. MERRILL.

THE NUTRITION FOUNDATION

GRANTS amounting to \$258,100 were made at the third anniversary meeting on March 29 of the Nutrition Foundation. These cover six new research projects and twenty-four renewals of grants for studies now under way.

The grants made were as follows:

Harvard University received \$47,500 for a five-year study of nutrition in relation to maternal health, which will include the problems of pregnancy, childbirth, the condition of the newborn infant and its subsequent health and development.

The Public Health Research Institute of the City of New York received \$10,500 for a three-year study of the development of microchemical methods for evaluating nutritional status. There is a critical need for practical methods of measuring the influence of nutrition on public health in the marginal zones of malnutrition.

The School of Medicine and Dentistry at the University of Rochester received \$10,000 for a two-year study of the nature and significance of a pigment associated with vitamin deficiency.

Grants were also made to the University of Pittsburgh for studies on self-selection of diets; to Washington University for studies on the mechanism of carbohydrate reactions in animal tissues, and to the University of Wisconsin to study the characterization of compounds containing bound biotin and other vitamins.

Renewals were made of grants previously made to nineteen universities and institutions, including Stanford University, the University of Wisconsin, the University of California, Columbia University, Duke University, the University of Illinois, the University of Cincinnati, Ontario Agricultural College, Purdue University, the University of Arkansas, Oregon State College, the University of Toronto, New York University, the Oklahoma Experiment Station, Vanderbilt University, Yale University, Harvard University, Cornell University and the National Research Council.

THE American Institute of Nutrition has awarded the Mead Johnson and Company Prize for 1945 to Dr. Dillworth Wayne Woolley, of the Rockefeller Institute for Medical Research, in recognition of his studies on the inhibitors of vitamins and for his many contributions to the identification of nutritional factors of a vitamin nature. The Borden Award in nutrition has been conferred on Dr. H. H. Mitchell, of the University of Illinois, in recognition of his outstanding contributions, which have emphasized the nutritive significance of the components of milk and of dairy products.

PENNSYLVANIA ACADEMY OF SCIENCE

At the recent annual meeting of the Executive Council of the Pennsylvania Academy of Science held in Philadelphia, the following officers were elected for the coming year:

President, Dr. Bradford Willard, chairman of the geology department, Lehigh University, Bethlehem, Pa.

President-elect, Dr. T. L. Guyton, state entomologist and chief of the Bureau of Entomology, Department of Agriculture, Harrisburg, Pa.

Vice-president, Eastern Division, Dr. H. K. Lane, professor of science, Hershey Junior College, Hershey, Pa.

Vice-president, Western Division, Miss Marie B. Knauz, teacher of biology, Pittsburgh, Pa.

Secretary-Treasurer, Dr. V. Earl Light, professor of zoology, Lebanon Valley College, Annville, Pa.

Editor of Journal, Dr. R. M. Foose, senior geologist, Pennsylvania Geological Survey, Harrisburg, Pa.

Editor of Pennsylvania Science News-Letter, Charles E. Mohr, director of education, Academy of Natural Science, Philadelphia.

Junior Academy of Science Sponsor, Miss Sophie Moiles, teacher of physics, Johnstown, Pa.

The regular annual Easter meeting of the entire membership of the academy, scheduled for Juniata College, was cancelled by request of the War and Navy Departments. Acting officially in its place the executive council met for two days to transact the necessary business. Membership of the academy increased over ten per cent. this year and now numbers over five hundred members, coming chiefly from the colleges, universities, high schools and research laboratories of the state. The next annual Easter meeting of the academy will be held at Lehigh University, if transportation regulations permit.

JOHN C. JOHNSON, *Press Secretary*

SCIENTIFIC NOTES AND NEWS

At a meeting of the Society of Experimental Psychologists, Inc., the Howard Crosby Warren Medal was awarded to Professor Clark L. Hull, of Yale University. The citation reads: "For his careful development of a synthetic theory of behavior. This theory has stimulated much research and it has been developed in a precise and quantitative form so as to permit predictions which can be tested empirically. The theory thus contains within itself the seeds of its own ultimate verification or of its possible final disproof. A truly unique achievement in the history of psychology to date."

PROFESSOR ISIDOR ISAAC RABI, of Columbia University, received the 1944 Nobel Prize in physics on April 11, in recognition of his research in the resonance method of registering the magnetic quality of atoms. The presentation was made by President Nicholas Murray Butler at a reception in the Men's Faculty Club, given in honor of Professor and Mrs. Rabi by Dr. Frank Fackenthal, provost of the university.

THE Society of Automotive Engineers at its recent meeting in New York City presented the Wright Brothers Medal to Dr. Kenneth Campbell, research engineer of the Wright Aeronautical Corporation, in recognition of his paper read before a meeting of the society last year on "Engine Cooling Fan Theory and Practice."

THE Chancellor's Medal of the University of Buffalo was presented on February 22 to Dr. Nelson Gorham Russell, emeritus professor of medicine of the university, with the inscription, "in recognition of a lifetime of eminent service to your profession, your community and your country which has 'dignified Buffalo in the eyes of the world.'" Dr. Russell, who has been associated with the School of Medicine for fifty-three years, was also cited as a "scientist, soldier, civic leader, teacher and mentor of two generations of physicians and dean of the practitioners of the healing art in this region."

THE *Bulletin* of the American Mathematical Society reports that Professor Marston Morse, of the Institute for Advanced Study at Princeton, N. J., has received citation from the War Department for "outstanding performance as a civilian in government work in furtherance of the nation's war effort."

DR. W. D. CAIRNS, professor of mathematics, emeritus, of Oberlin College, was recently elected honorary president for life of the Mathematical Association of America. Dr. Cairns was for twenty-seven years secretary-treasurer of the association and for the past two years president.

DR. ALBERT EINSTEIN has retired from active duty on the staff of the Institute for Advanced Study at Princeton, N. J. The title emeritus has been conferred on him.

DR. EDWARD C. ELLIOTT, for the past twenty-two years president of Purdue University, will retire at the end of June.

IN a reorganization of biology at Harvard University, Professor Alfred S. Romer, professor of zoology and curator of vertebrate paleontology, has been appointed director of the Biological Laboratories, and Professor E. S. Castle, of the department of physi-

ology, has been made chairman of the department of biology.

DR. WARREN WHEELER, who for the past three and a half years has been director of the bacteriological and chemical laboratories of the Detroit Children's Hospital and assistant professor of pediatrics at the College of Medicine of Wayne University, has been appointed associate professor of pediatrics at the Ohio State University.

THE *British Medical Journal* reports that Dr. H. A. Krebs has been appointed to the newly established chair of biochemistry at the University of Sheffield. Professor Krebs, who graduated in medicine at the University of Hamburg in 1925, first went to Sheffield from a post at Cambridge nine years ago and, as lecturer, founded a school of biochemical research which has received support from the Medical Research Council and from the Rockefeller Foundation of New York. He has taken an active share in organizing nutritional studies for the Government during the war.

DR. TOMLINSON FORT, professor and head of the department of mathematics of Lehigh University, has resigned in order to become professor of mathematics and head of the department at the University of Georgia.

RALPH R. BEAL, for nine years research director of the Radio Corporation of America, has been elected vice-president in charge of engineering of RCA Communications, Inc. For the last two years Mr. Beal has been a member of the microwave committee of the National Development Research Committee of the Office of Scientific Research and Development.

DR. KARL F. LAGLER, assistant professor of zoology at the University of Michigan, has been given the additional title of research associate of the Laboratory of Vertebrate Biology. He will be in charge of researches concerned with fish biology.

DR. LEO M. DAVIDOFF has been appointed as of September 1 attending neurological surgeon at Montefiore Hospital, New York. He has also been promoted to a professorship of clinical neurological surgery at the College of Physicians and Surgeons of Columbia University. He will continue his consulting practice in neurosurgical problems at the Montefiore Hospital.

DR. HERBERT R. UNSWORTH, clinical assistant professor of neuropsychiatry at the School of Medicine of the Louisiana State University, has been appointed attending specialist in neuropsychiatry at the U. S. Marine Hospital in Carville, La.

DR. JOHAN BJORKSTEN, chemical director of the Quaker Chemical Products Corporation, Consho-

hocken, Pa., has established an independent research organization with headquarters in Chicago.

DR. ROBERT M. KLEINPELL, micropaleontologist of Bakersfield, Calif., and of the California Institute of Technology, who was captured in Manila by the Japanese in 1942, has recently been released from prison and has returned to the United States.

DR. EDMUND V. COWDRY, professor of anatomy at the Washington University School of Medicine, St. Louis, will deliver on April 23 the Adam M. Miller Memorial Lecture of the Long Island College of Medicine. He will speak on "Microscopic and Chemical Properties of Precancerous Lesions."

DR. K. LARK-HOROVITZ, head of the department of physics of Purdue University, addressed the Chapter of Sigma Xi of the University of Cincinnati on April 2. His subject was "Radioactive Tracers and their Application to Biological Problems."

PROFESSOR JOHN G. KIRKWOOD, of Cornell University, will deliver the Edward Lee Memorial Lecture at the University of Chicago on May 11. He will speak on "Hindered Molecular Rotation in Polar Liquids."

COLONEL SCOTT B. RITCHIE, deputy chief of Research and Development Service, Office Chief of Ordnance, discussed "Army Ordnance Development Since World War I" at a meeting on April 12 of the Washington, D. C., Section of the American Society of Mechanical Engineers.

DR. ERIC G. BALL, associate professor of biological chemistry at Harvard University, is giving a series of lectures at the School of Medicine of the University of Brazil in Rio de Janeiro, where he will direct research in cellular respiration. His trip is under the joint auspices of the Brazilian Government and the Department of State of the United States.

PROFESSOR JAMES MACKINTOSH, dean of the London School of Hygiene and Tropical Medicine, has been lecturing in Sweden for the British Council on "Housing and Medicine," "Nutrition and Medicine" and other aspects of social medicine and health education.

THE Director-General of the Army Medical Services, Sir Alexander Hood, K.C.B., will deliver on May 28 the Harveian Lecture at the Royal College of Surgeons of England. He will speak on "Total Medicine."

THE annual general meeting of the American Philosophical Society that it was planned to hold on April 19, 20 and 21 in Philadelphia, has been cancelled in compliance with the request of the War Committee on Conventions of the Office of Defense Transportation.

IN compliance with the wishes of the ODT, the Institute of Chemists has cancelled its annual meeting, and its Gold Medal will be awarded by the Miami Valley Chapter at its regular meeting in Columbus, Ohio, on May 11. As already announced in *SCIENCE*, the 1945 recipient of the medal is John W. Thomas, chairman and chief executive of the Firestone Tire and Rubber Company. William M. Jeffers, president of the Union Pacific Railroad and formerly U. S. Rubber Director, will address the meeting. Other speakers will include Dr. Hezleton E. Simmons, president of the University of Akron; Dr. Donald B. Keyes, director of the Office of Production, Research and Development of the War Production Board; John D. Coleman, president of the Dayton Society of Professional Engineers and supervisor of Production Processes of the Frigidaire Division of the General Motors Corporation. The medal will be presented by Dr. Gustav Egloff, president of the institute.

THE regular meeting of the Paleontological Research Institution took place on April 7 at Ithaca, N. Y. Special orders of the day for which appropriations have already been made related (1) to the early completion of doubling the library and working space of the institution; (2) to a discussion of the best methods for finishing publication of (a) a 50 plate volume on Ordovician cephalopods, (b) a 65 plate volume on Jackson Eocene Mollusca, (c) completion of the gathering together of material and the publishing of Carpenter's West Coast molluscan types and (d) minor reports.

DISCUSSION

"FACTS" AND "INTERPRETATIONS" REGARDING RACE DIFFERENCES

IN a recent note,¹ Mr. Birch objects to the distinction which I drew between "facts" and "interpretations" with respect to psychological differences among races. I would agree that the distinction breaks down

in those rare cases in which only one interpretation is possible. But such rigid relationships are seldom found in the social sciences; inevitably, perhaps, there are several interpretations of the same data and one interpretation is apt to be more adequate than another. I shall give two "exhibits" as illustrations of what I mean.

Exhibit A: Medians on the Army Alpha and Army

¹ *SCIENCE*, n.s., 101: 173-174, 1945.

Beta Tests achieved by Whites and by northern Negroes in 1918 were as follows:

TABLE 1

	Alpha median	Beta median
White	58.9	43.4
Northern Negro	38.6	32.5
Difference	20.3	10.9

Alpha, it will be remembered, was a "verbal" or language test requiring the ability to read and write. Beta was a non-language test; the subject did not have to read or write but simply indicated his answers by marking. In presenting the above data as evidence that the Negro was handicapped in Alpha by lack of schooling, Professor Klineberg² writes: "The discrepancy (in Beta) is still marked, but it has been considerably reduced. It is obvious that the language factor is not the only one responsible for the observed difference, but it seems clear that it does participate in the final result." I am not concerned with Klineberg's general conclusion as to the effect of schooling, which is very probably true, but only with his statement that the difference between Negroes and Whites is "considerably reduced" upon Beta. Since Alpha and Beta were scored in different units, a 20-point difference on Alpha can not be compared directly with a 10-point difference on Beta. Moreover, if the proportion of northern Negroes who exceeded the White medians in Alpha and in Beta are computed, it is found that 29 per cent. of Negroes exceeded the White median on Beta as compared with 27 per cent. who exceeded the White median on Alpha. The difference in median performance of Whites and Negroes is not, therefore, "reduced" on Beta as compared with Alpha, and Klineberg's interpretation of the "facts" of Table 1 is clearly in error.

Exhibit B: In their recent pamphlet, "Races of Mankind," Benedict and Weltfish³ reproduce the Alpha medians of Negro soldiers from New York, Illinois and Ohio, and of White soldiers from Arkansas, Kentucky and Mississippi (see Table 2).

TABLE 2

Whites			Negroes		
State	N	Median	State	N	Median
Arkansas	618	41.0	New York	850	44.5
Kentucky	832	41.0	Ohio	152	48.8
Mississippi	665	40.8	Illinois	578	46.9

From the context it is quite clear that these data

² O. Klineberg, editor, "Characteristics of the American Negro," N. Y., Harper & Bros., 1944, p. 58.

³ R. Benedict and G. Weltfish, "The Races of Mankind," Public Affairs Pamphlet, 85, Public Affairs Comm., 1943.

are meant to be taken as evidence that there are "no race differences." These authors fail to state, however, (1) that their data represent extreme selections and (2) that when Negroes in New York, Ohio and Illinois are compared with Whites in the same states, the overlap is 28 per cent.—almost exactly what it was in the country as a whole. One might argue, therefore, that given better schooling the Negro does indeed improve his Alpha score—but not his position relative to the White. Again, one might argue in favor of race differences on the grounds that White southerners did as well as highly selected northern Negroes in spite of educational handicaps which unfortunately affect Whites as well as Negroes. Divergent interpretations like these support by contention that the same facts can be marshalled equally well to support opposing points of view. Incidentally, the authors of "Races of Mankind" omit the sizes of their samples (shown in Table 2), which in the case of the Ohio Negroes might have cast some doubt upon their conclusions.

Although in my note I did not support any particular view of race differences, Mr. Birch feels constrained to defend vigorously the "no differences" view. As a first exhibit, he cites Brigham's⁴ so-called "rejection" of his own test findings with regard to natio-racial differences. This is not an especially happy choice of evidence for Mr. Birch's purposes. When Brigham wrote his paper fifteen years ago, he was greatly impressed by the demands of the factor analysts for "purity" within a test battery. Brigham believed that Alpha was not "factorially pure," and was inclined to discount his findings with the Alpha test on that account. In my opinion, Brigham attached too much importance to the matter of test purity and Klineberg too uncritically accepted what he calls Brigham's "recantation." As a matter of fact, we know that the Alpha test possesses considerable generality (intercorrelations range from .59 to .86), as much, indeed, as does the new Stanford-Binet. Brigham's results may be criticized on the score of sampling and for other reasons; but the question of test purity upon which he based his "rejection" is not a crucial issue. As a final argument, Mr. Birch quotes a statement from Professor Klineberg⁵ to the effect that "The conclusion (*i.e.*, that there are race differences) came first and the facts were found to justify it." In many instances this has undoubtedly been true. But the reversal of facts and conclusions is not peculiar to the race differences advocates, as is amply demonstrated by Professor Klineberg's own book.

⁴ C. C. Brigham, *Psychol. Rev.*, 37, pp. 158-165, 1930.

⁵ O. Klineberg, "Race Differences," Harper and Bros., N. Y., 1935.

Like many anthropologists, Professor Herskovits is concerned over the psychologist's use of the term "race." In commenting on my brief note,⁶ Professor Herskovits offers an original—to say the least—if somewhat whimsical solution to the whole problem of race differences. One can not, he writes, speak of race differences as between Negroes and Whites, since there is no Negro race in this country, but instead a group of more-or-less African ancestry. At first glance, this view seems reasonable enough, though upon examination it is clearly a quibble over terms. Surely a group does not have to possess unmixed ancestry (be racially "pure") before the term "racial" is applicable. The anthropologist speaks understandably enough of the factor of economic status, although the economic condition within a given group is never a constant and may vary widely. By the same token, the psychologist may speak sensibly of the factor of race when the group being described does not possess biologically pure ancestry. Except for small groups of transitional types, the American Negro constitutes a recognizable and clearly defined group; and the criterion of membership in this group is (more-or-less) African ancestry. To repeat what I said in my note, studies in this country over the past forty years have regularly and consistently found differences as between the American Negro and the American White. These differences, to be sure, are subject to a number of interpretations; but the fact of their existence can not be denied.

Although the extent of race mixture in this country has probably been fairly large, I do not believe that Professor Herskovits's oft-quoted estimate of the degrees of admixture possesses much validity. Professor Herskovits writes that "... it would be hazardous to place the proportion of those among the American 'Negro' population of unmixed African descent—that is biological Negroes—at more than 30 per cent., with the large probability of a much smaller percentage of unmixed Negroes to-day." In view of the method by which this figure was obtained, I think it would be hazardous to accept it as anything more than a guess. Incidentally—and finally—I hope that Professor Herskovits, having now clarified the term "race" for psychologists, will proceed to clarify the even more nebulous concept of "culture" for his fellow anthropologists.

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MARINE FOULING AND ITS PREVENTION

THE fouling of ships reduces their speed, curtails their availability (by dry docking) and increases expenditure on fuel and labor. The 1943 report, issued

⁶ SCIENCE, n.s., 101: 200.

by the joint anti-fouling subcommittee of the Iron and Steel Institute and the British Iron and Steel Federation, deals mainly with the anti-biotic properties of the copper and mercury components of protective paints.¹ The report shows that the progressive reduction in the effectiveness of such paints is not due to the exhaustion of their metallic constituents but to the blocking of their active surface by slimy or cement-like formations largely of organic origin. This evolution of a natural "antidote" by marine organisms demands the revision of the whole idea of organic and inorganic poison paints as applied to fouling. The significance of these marine deposits becomes still more apparent when it is realized that they are not only products of marine life, but form the habitat or anchorage of many other types of flora and fauna. As to the nature of the slime, cement, coral and shell formations—they consist of organic silicious and calcareous material in which the last predominates, whilst the first stabilizes the colloidal state, as in the case of mother of pearl. Again, apart from quantitative considerations, the ability of calcium carbonate to exist as a colloid in an inorganic medium (in its liquid-colloid-solid transition cycle)² gives it a dominant place in our problem. This is borne out by the effect of the "cleaning ports," treatment of dock basins infested with calcareous organisms, the geology of the area and such chemical influences as dissolved carbon dioxide, "soft" water, etc., upon the extent and type of fouling.³ It would therefore appear that any measure calculated to hinder or inhibit the deposition of calcium salts would automatically counteract fouling. Such a preventive measure may possibly be found in a paint, incorporating zeolite or sodium permutit in a suitable medium, to be applied on top of the usual anti-corrosion protective covering. We may expect here the automatic combination of the two distinct processes operating in water-softening, namely, (1) the conversion of the sodium aluminium silicate into calcium aluminium silicate in presence of calcium bicarbonate and (2) the regeneration of the sodium aluminium silicate by the action of sodium chloride upon the calcium aluminium silicate. In other words, the normal salinity of seawater, exceeding its calcium carbonate content, may be sufficient to protect the anti-fouling paint and prevent the deposition of calcium carbonate. Such catalytic action would leave the sodium permutit paint apparently unaffected, with its surface free from gelatinous calcium carbonate.

¹ See also Bengough and Sheppard, Paper No. I on "The Corrosion and Fouling of Ships," published by the Iron and Steel Institute, 1943; Ewart Bowles v Bengough, *Nature*, 152: 159, 1943.

² M. Copisarow, *Jour. Chem. Soc.*, 123: 785, 1923; 222, 1927; *Kolloid Zeits.*, 49: 309, 1929.

³ *Idem*, *Chem. and Ind.*, Nov. 18, 1944.

In the case of siliceous adhesions counteracting measures could perhaps be found by incorporating aluminium powder or aluminium hydroxide in the anti-fouling paint. The former may be expected to function as a mechanical buffer against diatomaceous matter (compare with silicosis treatment), whilst the latter could probably exert a neutralizing action upon sponge products (silicic acid gels) in their transition to flint-like consolidations.

In addition to regular progressive fouling we have adventitious attachment of floating seaweed. Under the influence of mechanical forces drifting vegetation is anchored mainly on the ship's sides, the types of algae being graded into zones by photochemical action. This raises the question, whether biological orientation and even inhibition could not be attained by the control of refraction or absorption of selected spectrum bands (basic to such photochemical activity), which may be affected by the addition of specific pigments or luminescent materials to the anti-fouling paint. Again, a paint containing zeolite and any of the already tried anti-biotics or such agents as boron compounds and prussian blue could perhaps be useful in this direction.

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ENCYSTMENT AND EXCYSTMENT IN CHAOS

IN September of 1942, a fairly strong culture of *Chaos chaos* Schaeffer, in 200 cc of Hahnert's solution, was accidentally misplaced, and when examined about two months later no active forms were observed. Instead, about 600 small white translucent spheres, that looked like cysts, were found. These were relatively small, measuring, as a rule, from 80 to 150 micra in diameter.

A few hundreds of the cysts were selected with a micropipette and placed in fresh Hahnert's solution together with concentrated paramecia and chilomonas, and after about a week, two active specimens were observed. In other experiments it has taken at times from four to eight weeks and even longer for excystment. The smallest observed excysted forms were 500 to 550 micra when expanded. Only a very small percentage of the cysts excysted.

Encystment can be induced by lack of food. After the food has disappeared, chaos gradually gets smaller, but this reduction in size may be due in part to fission, as division has been observed in specimens placed in food-free Hahnert's solution up to ten days after transfer. As the size decreases, chaos occasionally takes on a spherical form, then returns to its characteristic shape. Eventually it gets so small that it may be mistaken for an ameba. Stained specimens show that there has been a constant reduc-

tion in the number of nuclei. The smaller forms usually have fewer than twenty nuclei, and in 41 specimens with fewer than twenty nuclei, six had between eleven and twenty, fourteen between six and ten, ten between three and five nuclei, but eleven showed no nuclei, though the last may have been due to weak staining.

These small forms eventually roll up into a sphere and encyst. Stained specimens of cysts show one nucleus as a rule, but thickness of the cyst wall makes staining difficult. This wall is crystal clear in living specimens, but sections of cysts show that it is a double structure.

Since excystment is rather rare, only two specimens were killed and stained. These showed about 25 nuclei. At present, work is in progress to discover how the number of nuclei is reduced before encystment, how the number is restored, together with other nuclear changes occurring during the process of encystment.

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AN INORGANIC "BOUNCING PUTTY"

CONCENTRATED sodium silicate solutions show nearly the same physical properties as "bouncing putty."¹ A sample of sodium silicate, accidentally obtained from a more dilute solution by slowly drying for several months, containing 10.9 per cent. Na_2O , 35.0 per cent. SiO_2 and 54 per cent. H_2O , exhibited these properties to a remarkable degree. The material was a transparent rubbery liquid. It could be shaped into a ball and bounced off a hard surface like so much rubber. It was not sticky, and a ball formed from it could be bounced off a surface of the same material. A lump of this concentrated sodium silicate flattens out slowly if it is placed on a table top. In five minutes a ball of the material one inch in diameter will have a surface of about $\frac{1}{4}$ inch diameter in contact with the flat surface. It will wet glass or ceramic materials if given a few minutes to do so, but will not wet them instantaneously. The sodium silicate solution retains its ability to bounce indefinitely provided it is protected from loss of water by evaporation; but it dries out in a few hours when exposed to air, becoming brittle and losing its ability to bounce.

The viscosity of this material was determined in an apparatus used for the determination of the viscosity of molten glass. The apparatus uses the concentric cylinder method. The viscosity was found to be 1.1×10^6 poises at 19°C . and was independent of the rate of shear.

D. L. HANNA

ILLINOIS STATE GEOLOGICAL SURVEY

¹ *Chem. and Eng. News*, 22: 2016, 1944.

SCIENTIFIC BOOKS

THE PHYSICS OF MUSIC

The Physics of Music. By ALEXANDER WOOD. 255 + xii pp. 91 figs. 109 illustrations. 5 $\frac{1}{4}$ " x 8 $\frac{1}{4}$ ". Cleveland, Ohio: The Sherwood Press. 1944. \$8.00 (special introductory price \$6.00, subject to cancellation without notice).

Dr. Wood offers both the musician and the physicist a very stimulating review of the literature dealing with the newer research in the physical basis of music. With the rich experience gained in writing a number of texts in this interesting borderline field, the author approaches the subject on quite an unsophisticated level in the present volume. This is, no doubt, done to avoid discouraging the musician. Yet the physicist can also profit by the great number of references to situations beyond the stage of first approximations of the classical literature on the subject. Probably the most startling innovation from the point of view of the physicist is the inclusion of a considerable treatment of the psychology of music.

The bibliography is very representative of both the foreign as well as the American publications. Above all, the *Journal of the Acoustical Society of America* can be justly proud of the many references made to its contributors. The author skilfully interweaves the diverse bits of research accomplished during the past decade or more into a well-integrated and meaningful pattern, nicely augmenting the classic experiments of the pioneers in this borderline field.

Even the despised musicologist (for the term seems to cover a multitude of sins) comes in for a share of glory in the very desirable consideration of the esthetic values in music. To cap the climax, notions of musical expression and interpretation are even suggested in the chapters on voice and orchestral instruments. There are, it must be admitted, a few naive confusions of terminology in these chapters on musical instruments which do not, however, detract materially from the good of the whole. The author's friends in the musical world will take keen delight in pointing out these errors to him, and future editions of the book can correct them. The hope of finding a few such mistakes should challenge the musician to read the text more carefully.

The "Acoustics of Auditoriums" is a chapter especially well treated. This particular chapter includes a wealth of illustrative material. In fact, the entire text is extraordinarily well supplied with diagrams and various types of illustrations. It is for this very reason, probably, that this volume costs as much as \$8.00. This is really too much to have to pay for a beginner's text-book, and it may unfortunately serve to limit the output to the chain of interested libraries. Yet it is a book well worth while owning.

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ABE PEPINSKY

PSYCHOPATHOLOGY

Contemporary Psychopathology—a Source Book. Edited by SILVAN S. TOMKINS. Cambridge, Mass.: Harvard University Press, 1943. \$5.00.

THE volume consists of a group of forty-five articles collected from the general literature and reprinted in book form. In the editor's words, "This volume is designed for courses in abnormal psychology. Recent advances in research, while essential to a comprehensive understanding of the field, generally appear in journals not readily accessible to undergraduate readers." The value of this compilation of articles is, therefore, dependent upon the significance of the individual contributions and the integration of the forty-five subjects as representative of the current viewpoints of psychopathology.

In a short introduction Dr. Henry A. Murray says:

The attempt at comprehensiveness has been abandoned; a great many topics are utterly ignored. Only areas that have been illumined in recent years by clarifying hypotheses or the acquisition of new facts are given place, and these are treated with specialized attention.

Dr. Tomkins has been willing to let the authors of the several papers talk for themselves. He has not added any comment, criticism or explanation, but merely presented his choice of material.

The forty-five papers are divided into four groups: (1) Mental Disease in Childhood; (2) Psychoneuroses and Psychosomatic Medicine; (3) Schizophrenic Psychoses; (4) Experimental Psychopathology.

One is struck at once by the fact that the contributions are in large part related to medical problems. While this may be quite reasonable in considering a subject such as psychopathology, one nevertheless may wonder whether medical orientation is the most appropriate one for students in abnormal psychology, made up in very large part presumably of people of college undergraduate status. In fact, approximately two thirds of the articles are presented by members of the medical profession. Does this mean that there is a paucity of fine work being done by psychologists not possessors of the M.D. degree?

Remaining in a critical attitude, one can but regret the editor's decision: "Many recent developments in psychoanalysis, anthropology and sociology had to be excluded. Therapy in the psychoses other than schizophrenia has received scant attention."

Possibly another criticism might be leveled at the absence of any appended bibliography to the articles selected. This may be a criticism of some moment in view of the fact that many of the articles deal with subjects about which there is contention or dif-

ference of viewpoint, and therefore the presentation without either any discussion on the part of the editor, the presentation of another viewpoint in the form of an article differently oriented or bibliographic references may do an injustice to the subject-matter.

It is indeed no simple matter to select out of the enormous literature which the general subject-matter covers forty-five articles and say that these are the important contributions to psychopathology. In view of this difficulty the reviewer must give full-hearted appreciation of the articles selected. There is none that is not important, that is not stimulating and that does not make good reading.

It seems hardly reasonable to pick out any special

article either for commendation or in criticism of its selection, nor would it be profitable to attempt to discuss one or a few of the forty-five contributions.

The reviewer would, therefore, conclude that it is worth the time of any physician interested in psychopathology and medical students to read most, if not all the articles in this compilation. Graduate students of psychology and some mature undergraduates will also benefit from a study of the selections, but should realize that many of the articles need medical experience to be properly evaluated.

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REPORTS

OVER-THE-COUNTER SALE OF SULFONAMIDE DRUGS¹

SECTION 118 of the Sanitary Code of the City of New York prohibits the sale of sulfonamide drugs, whether for internal or external use, without a prescription. Two groups of sulfonamide preparations are excepted, namely, sulfonamide nose drops containing not more than 2.5 per cent. of sulfa drugs and adhesive bandages containing sulfathiazole. (The usual concentration of the drug in these bandages is 6 to 12 mgms of sulfathiazole per inch of bandage.) These two exceptions have been allowed because (1) the Federal Food and Drug Administration permits the over-the-counter sale of these products, provided they bear proper precautionary labels and directions for use, and (2) at the time this ruling was made there was no actual proof that the external use of the sulfa drugs in small concentrations had caused any harm.

For advertising reasons sulfa drugs are now being added to various kinds of salves, tape bandages, shaving creams and similar products which are sold under well-established proprietary names. Of late, one of the drug concerns applied to the Health Department for permission to sell without prescription a suspension containing 5 per cent. of sulfathiazole. In view of this and in view of the fact that more and more reports appear in the literature to the effect that topical application of the sulfa drugs can arouse a hypersensitivity resembling an allergic phenomenon, the Commissioner of Health submitted to the committee the question of what action the Board of Health should take in this regard.

After looking into the matter carefully, your subcommittee has come to the conclusion that clinical experience bears out the fact that a small percentage

¹ Report of the Subcommittee of the Committee on Public Health Relations of the New York Academy of Medicine.

of people become sensitized to the sulfonamide drugs, that a severe systemic reaction may be produced in these persons when the drugs are administered a second time and that a minute amount of the sulfonamide drugs applied to the unbroken skin or to the mucous membranes can and does sensitize an individual even more quickly and with greater certainty than when such drugs are administered by mouth.

Because such sensitization is often dangerous and prevents the use of the sulfonamide drugs in treatment of conditions in which they are particularly indicated, because over-the-counter sale of preparations containing sulfonamides will expose many persons to the danger of sensitization, and because the wide use of the sulfonamide drugs in ineffective concentrations may result in an increase in organisms resistant to the sulfonamides, your subcommittee recommends that, until such time as clinical evidence may justify a contrary course of action, Section 118 of the Sanitary Code of the City of New York be so amended as to eliminate all exceptions from the clause which prohibits the over-the-counter sale of sulfonamide preparations.

The subcommittee wishes to suggest the desirability of making this recommendation known to the general public. The subcommittee likewise considers it desirable to issue a statement on the part of the Committee on Public Health Relations to the general profession warning against the unnecessary use of sulfonamide preparations for the treatment of minor conditions.

Respectfully submitted:

GEORGE MILLER MACKEE, *Chairman*

MCKEEN CATTELL

RUSSELL L. CECIL

ROBERT A. COOKE

E. H. L. CORWIN, *Secretary*

Approved by the Committee on Public Health Relations on March 5, 1945.

SPECIAL ARTICLES

THE FATE OF ESTROGENIC METAHORMONES IN THE LIVER¹

FULL evidence exists that inactivation of α -estradiol takes place in the liver. But there is still controversy as to the end-products of the metabolic changes ovarian estrogens undergo when metabolized in the liver (see the summary²). There is the fundamental problem whether urinary estrogens or metahormones, as estriol and others, are these end-products. We have undertaken a study of this question with two natural estrogens, as estriol and equilenin, and two artificial derivatives, as α - and β -dihydroequilenin. Our experiments, which are of long duration, give clear evidence that urinary estrogens, or their derivatives, are inactivated in the liver as ovarian estrogens are.

As has been shown in former work, abdominal fibroids similar to those induced with ovarian estrogens^{3,4} can be elicited with the mentioned four estrogens when quantities sufficiently great are absorbed from subcutaneously implanted tablets during three months.⁵ Will similar fibromatogenic quantities of urinary estrogens still elicit fibroids when absorbed from the spleen? Should estriol or equilenin be the end-products of hepatic antiestrogenic activity, these substances when again driven through the liver should invariably keep their estrogenic faculties, i.e., they should still be fibromatogenic when absorbed from intrasplenic tablets as they are when absorbed from subcutaneously implanted ones.

Tablets of estriol, equilenin, α - and β -dihydroequilenin of about 5 mm in diameter and weighing 18 to 48 mgr were implanted into the spleen of 51 castrated female guinea pigs which were autopsied 90 days later. The results were compared with those formerly obtained with subcutaneously implanted tablets of urinary estrogens.⁵ The fibrous abdominal reaction was classified according to units as explained in former papers (see especially⁶). Fibrous peritoneal strands or tumors of four different regions (uterus, mesosalpinx, digestive tract and abdominal

wall, spleen) are arranged for each region separately in four different classes according to their size, and each region is characterized by the values 0.5, 1, 2 and 3; the sum of the values of the four regions is the "fibrous tumoral effect" (F.T.E.); the maximal number of units an animal can attain is 12. Our results in the two comparative series are given in Fig. 1. A F.T.E. of 8 to 10 was attained with the

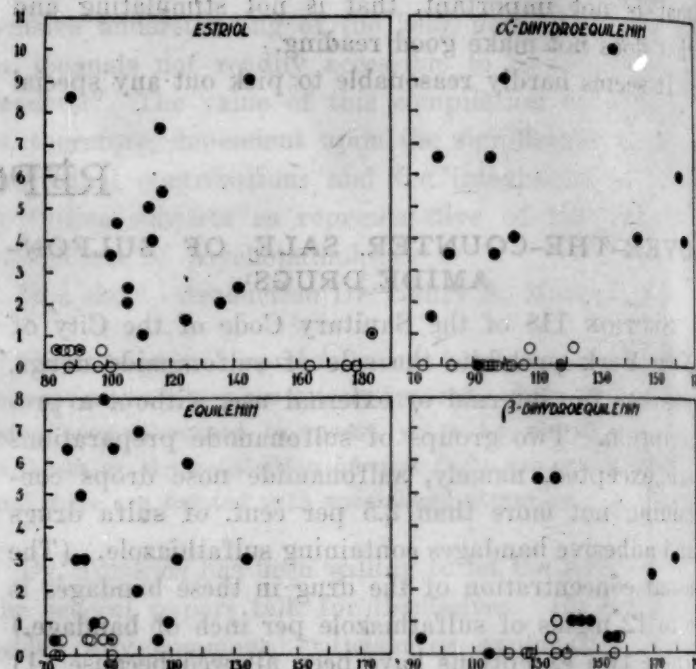


FIG. 1. Abscissa—quantities absorbed per day, in micrograms. Ordinates—units of F.T.E. ● Round black spot—animals with subcutaneously implanted tablets. ○ Circle—animals with intrasplenic tablets. ○ Circle with dot in the center—animals with intrasplenic tablets, with adhesions between spleen and abdominal wall.

absorption from subcutaneously implanted tablets of estriol, equilenin or α -dihydroequilenin; on the contrary, there was with the absorption of these substances from the spleen not a single animal with F.T.E. greater than 1, though the quantities absorbed were similar to and often greater than those which are fibromatogenic when absorbed from subcutaneously implanted tablets.

Our present work shows in a striking manner that the liver inactivates urinary estrogens even when enormous fibromatogenic quantities are absorbed from the spleen and administration is continued for several months.

As shown by the average uterine weight (see table) inactivation of urinary estrogens was not always a complete one; but the difference between the figures obtained with absorption from the spleen and with that from beneath the skin was enormous. There was

¹ Aided by grants from The Jane Coffin Childs Memorial Fund for Medical Research and The Rockefeller Foundation. Thanks are due for estrogens to Dr. Oliver Kamm, of Messrs. Parke, Davis and Co.

² J. Schiller and G. Pincus, *SCIENCE*, 98: 410, 1943.

³ A. Lipschütz and R. Iglesias, *C. R. Soc. Biol. (Paris)*, 129: 519, 1938.

⁴ A. Lipschütz, *Cold Spring Harbor Symp. Quant. Biol.*, 10: 79, 1942.

⁵ R. F. Mello, *Proc. Soc. Exp. Biol. and Med.*, 55: 149, 1944.

⁶ A. Lipschütz and M. Maass, *Cancer Research*, 4: 18, 1944.

TABLE 1

Estrogen used	Number of animals*	Absorbed per day μ gr	Uterine weight Average† gr	Uterine weight Range gr
Intrasplenic				
Estriol	10	115	1.6	0.6-2.7
Equilenin	12	80	0.9	0.5-2.8
α -dihydro-equilenin ...	10	97	1.1	0.3-4.4
β -dihydro-equilenin ...	10	137	0.8	0.4-2.2
Subcutaneous				
Estriol	15	111	5.4	3.7t.‡-15.0t.
Equilenin	15	97	6.8	3.8 -15.5t
α -dihydro-equilenin ...	12	111	7.5	3.6t. -15.1t.
β -dihydro-equilenin ...	12	136	5.9	3.4 -10.5t.

* Animals with adhesions of the spleen to the abdominal wall were discarded; see circles with dot in the center in the figure.

† 0.3 to 0.5 gr is the uterine weight of a castrated guinea pig; 1 gr is the approximate uterine weight of an adult virgin female.

‡ Uterine weight including subserous or mesometric tumors.

In the present series also an estrogenic action on the vaginal mucosa in about two thirds of the animals, though a weak one in most cases.

Our experiments leave no doubt about the statement that the liver is able to inactivate great quantities of estriol and equilenin. Though these urinary estrogenic metahormones derive from the intrahepatic conversion of ovarian estrogens, they are not the end-products.⁷ On the other hand, the question remains open about the special changes the metahormones undergo in the liver.

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THE FIRST STAGE OF ANTIGEN-ANTIBODY REACTION IN INFECTIOUS MONONUCLEOSIS

ONE of the problems posed following the description of the pathogenesis of erythroblastosis fetalis was the failure to demonstrate anti-Rh agglutinins in about 50 per cent. of the Rh- mothers of erythroblastotic infants.¹ It was assumed that in these indi-

*It has been shown in the meantime that also equilin is inactivated when injected into the spleen of the castrated rat. A. Segaloff, *Endocrinol.*, 33: 212, 1943.

¹P. Levine, L. Burnham, E. M. Katzin and P. Vogel, *Am. Jour. Obst. and Gyn.*, 42: 925, 1941.

viduals the antibodies are fixed to the tissue cells of the reticulo endothelial system.^{1,2} This view is no longer tenable because Race³ and Wiener⁴ independently described antibodies which unite with Rh+ blood but fail to cause visible agglutination.⁵ Presumably the Rh+ cells are specifically coated with the antibody so that they are no longer susceptible to the agglutinins in anti-Rh sera. These immune substances are referred to as blocking or incomplete antibodies. These significant observations were amply confirmed by the writers.

TABLE 1

THE SERUM USED IN THESE TESTS WAS DERIVED FROM A PATIENT SUFFERING FROM INFECTIOUS MONONUCLEOSIS

Blood	Agglutination titer	Hemolysis with complement*	Absorption for sheep blood†	Absorption for goat, type 3†	Blocking antibody‡
Goat, type 1	1:20	1:400	almost complete	none	strong
Goat, type 2	1:100	1:400	complete	complete	very strong
Goat, type 3	1:4000	1:400	complete	complete	
Sheep	1:2000	1:400	complete	none	

* Fresh guinea pig serum 1:10 was added. The values given indicate complete hemolysis.

† The agglutinating serum was diluted 1:20 and absorbed with one-half volume washed sediment of each of the four bloods. The absorbed fluids were tested with sheep blood and goat blood, type 3.

‡ Sheep blood was added after the initial readings were made.

It is known that the vast majority of the sera of patients suffering from infectious mononucleosis agglutinate sheep blood (Paul-Bunnell reaction). Exceptionally, however, this diagnostic reaction can not be demonstrated in otherwise typical cases.⁶ Apparently, this state of affairs is quite analogous to that existing in erythroblastosis fetalis.

Accordingly, a search was made of sera of patients in whom a diagnosis of infectious mononucleosis was established. A number of sera containing potent agglutinins for sheep blood were collected and one serum was found with almost complete lack of action on sheep red blood cells.⁷ This serum, however, contained antibodies which coated the surface of sheep

² A. S. Wiener, *Arch. Path.*, 32: 227, 1941.

³ R. R. Race, *Nature*, 153: 771, 1944.

⁴ A. S. Wiener, *Proc. Soc. Exp. Biol. and Med.*, 56: 173, 1944.

⁵ A similar effect with bacterial anti-sera was observed by A. F. Coca and M. F. Kelley, *Jour. Immunol.*, 6: 87, 1921.

⁶ N. Rosenthal and G. Wenkebach, *Klin. Woch.*, 12: 499, 1933.

⁷ For this specimen, I am indebted to Dr. J. H. Scherer, of the Medical College of Virginia.

blood, rendering these cells resistant to the action of active agglutinins. The blocking effect was distinct but not very potent.

However, strong support for the specificity of the reaction was derived from an unexpected source. In view of the close serological relationship of goat and sheep blood, an attempt was made to elicit the Paul-Bunnell reaction with goat instead of sheep blood. The first results with six different goat bloods apparently met with failure. But on extending the series to include an additional eight goats, three sorts of reactions were observed as indicated by the results summarized in Table 1.

With goat bloods of types 1 and 2 a strong blocking effect was obtained which is quite analogous to that observed in Rh - mothers of erythroblastotic infants. That specific union occurred is evident from the uniform results obtained in the hemolytic tests and the absorption effect on sheep blood.

It is of interest that sheep blood did not remove much of the agglutinin for goat blood, types 2 and 3. This suggests that for the serological diagnosis of infectious mononucleosis, selected goat blood may be preferable to sheep blood. It is still to be determined whether or not individual differences of sheep blood can be demonstrated with sera of patients suffering from infectious mononucleosis.^{7a}

A close analogy to the blocking effect obtained with goat blood or with Rh + blood is the specific absorption of phage by resistant strains of S. Stanley of the paratyphus B group as observed by Levine and Frisch.⁸ In other words, the first stage of the reaction—specific union—occurs in the absence of the visible effect of lysis.

The observations in erythroblastosis fetalis and infectious mononucleosis emphasize the practical and diagnostic importance of the first and far more fundamental reaction of antigen and antibody, i.e., specific union. At the same time, another source for the study of serological components of red blood cells is now available. Thus, one can no longer refer to agglutinable properties demonstrable by direct reactions as the only serological components of red blood cells.

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^{7a} Since this paper was submitted, Levine and Waller found individual differences in the agglutinability of sheep blood. However, selected goat bloods of the Nubian breed gave the highest titers.

⁸ P. Levine and A. W. Frisch, *Jour. Immunol.*, 30: 63, 1936.

ALTERATIONS IN THE ANTIDROMIC POTENTIAL OF MOTOR NEURONS FOLLOWING CHROMATOLYSIS¹

As a result of injury or virus invasion, neurons exhibit a wide range of pathological reactions. The best known of these and the one which is most easily controlled experimentally is retrograde chromatolysis. This may be produced in the spinal motor nuclei by cutting a peripheral nerve. Previous papers have shown that neurons so treated will not respond to the proprioceptive reflex² and that the motor neuron itself, rather than "presynaptic" elements, is the site of the deficiency.³ However, a gap still exists in the demonstration that the activity of the whole cell is changed by the chromatolysis, for it is possible that the phenomena depend upon alteration of parts of the cell in contiguity with the *boutons terminaux* of the primary afferent neurons. The data described below were derived from a series of eight experiments which rule out the participation of such local mechanisms in the development of the deficiencies of the chromatolysed cell.

Cats were prepared by cutting the right tibial nerve in the popliteal fossa and allowing the retrograde degeneration to reach its height. The technique of firing the motor neurons antidromically⁴ has been used in these experiments. The potentials of the cells, discharged without the mediation of other nervous elements, were recorded with a cathode-ray oscillograph. Systematic probing of the affected segments with micro-electrodes yielded data by which maps were made showing the distribution of the potential fields⁵ of both the degenerated tibial nucleus and its normal control on the other side of the spinal cord.

The normal antidromic potential (Fig. 1a) consists of two phases, a positive deviation which represents the approach of the conducted impulse to the cell⁶ and a second wave, of positive or negative sign depending on the spatial relations of the micro-electrode to the cells,⁷ which represents the discharge of the neuron. The early component is unaffected by degeneration, but the second or cellular component is greatly decreased in amplitude in the chromatolysed nucleus (Fig. 1b). As conditioning curves of the affected nuclei (plotting height of response to a second shock to the interval between two stimuli) show

¹ From the Department of Anatomy, University of Minnesota Medical School. Aided by a grant from the National Foundation for Infantile Paralysis.

² B. Campbell, *Science*, 98: 114-115, 1943.

³ *Idem*, *Anat. Rec.*, 88: 25-33, 1944.

⁴ J. C. Eccles, *Proc. Roy. Soc.*, B107: 557-585, 1931.

⁵ B. Campbell, *Anat. Rec.*, 91: 77-88, 1945.

⁶ R. Lorente de N6, *Jour. Neurophysiol.*, 2: 402-464, 1939.

⁷ Descriptions of the potential fields will be published elsewhere.

an elongation of the refractory period and no hint that some of the cells may be stimulated subliminally, we may assume that the decrease in the potential size

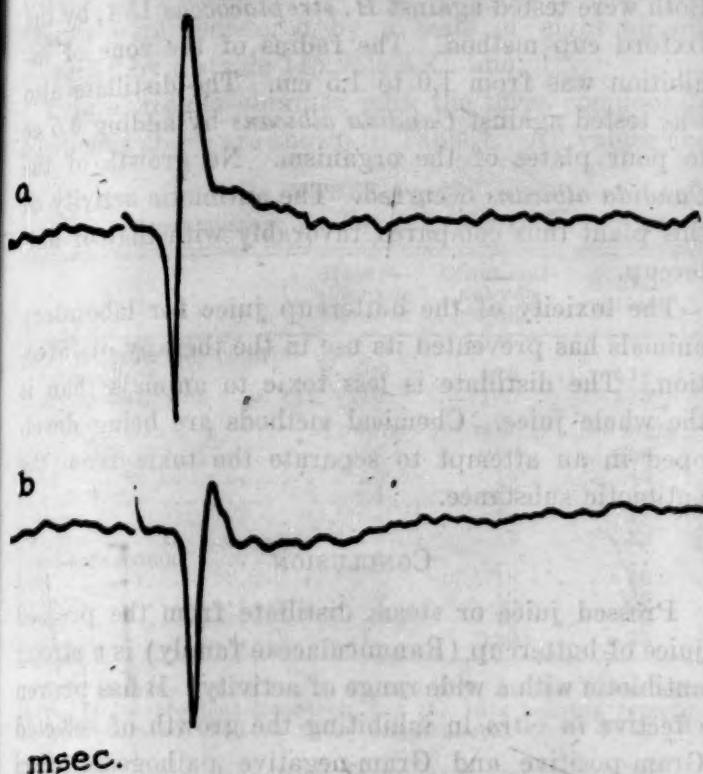


FIG. 1. Potential of ventral horn evoked by stimulation of tibial nerve and recorded by micro-electrode in 7th lumbar segment of the spinal cord of a cat. a. Normal side. b. Side altered by 27 days retrograde degeneration.

is due to a decrease in the external potential fields of the component cells of the nuclei. That this is apparently the equivalent of the loss of the ability to transmit the proprioceptive reflex offers a new field for theoretical consideration of interneuronal transmission.

CONCLUSION

Cells which have undergone chromatolysis as a consequence of peripheral nerve section respond to antidromic stimulation with reduced action potentials. No indication that any of the cells are stimulated subliminally is found in conditioning curves and the conclusion is drawn that the evoked external potential field of the individual cells is decreased in amplitude.

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THE ANTIBIOTIC ACTIVITY OF EXTRACTS OF RANUNCULACEAE¹

SINCE the observation of Boas² that the leaves and stems of some species of Ranunculaceae contain a

soluble substance which retards the growth of *Rhizopus nigricans*, several reports on the antibiotic properties of this family of plants have appeared.^{3, 4, 5, 6} Extracts from two members of the Ranunculaceae (buttercups and *A. pulsatilla*) have been examined for their antibiotic activity in this laboratory.⁷ Two types of extract were prepared as follows from buttercup, the plant more extensively investigated. The first, referred to as juice, was obtained by grinding the stems, leaves and blossoms of freshly picked buttercups in a meat-chopper, adding an equal weight of distilled water, mixing thoroughly and pressing out the juice through gauze. This was then autoclaved at 15 pounds pressure for 20 minutes. The fluid so obtained was greenish-brown, opalescent and of approximately pH 6.5. The second type of extract, referred to as distillate, was obtained from the juice by steam distillation. The distillate was water clear and of approximately pH 7.1.

The two types of buttercup extracts have been tested for their antibiotic activity against a number of cocci and bacilli which are listed in Table 1. The

TABLE 1

THE ANTIBIOTIC ACTIVITY OF BUTTERCUP JUICE AND DISTILLATE TESTED BY THE OXFORD CUP METHOD ON A SELECTED GROUP OF BACTERIA

Bacterium	Radius of inhibition of growth	
	Juice cm*	Distillate cm*
<i>H. strep.</i> 15A	0.9-3.0	1.5-3.0
<i>H. strep.</i> C203	1.5-2.2	1.1
<i>Strep. viridans</i>	0.9	0.5-1.5
<i>Pneumococcus</i> I	1.6	1.5
<i>Pneumococcus</i> III	0.6-1.0	0.6-1.0
<i>N. catarrhalis</i>	1.8	3.0
<i>Staph. aureus</i> BD	0.6	1.0
<i>Staph. aureus</i> Pos.	0.4	0.3-0.6
<i>Staph. Oxford</i>	0.5	1.0
<i>B. anthracis</i>	0.6	0.3-0.6
<i>Pseudomonas aeruginosa</i> (<i>B. pyocyaneus</i>)	0.6	0.5-0.7
<i>S. schottmüller</i> (<i>B. paratyphosus</i> B)	0.5	0.6
<i>Esch. coli</i>	0.5	0.5

* The figures given represent the minimum and maximum radius of inhibition observed. When penicillin was employed as a control utilizing *H. streptococcus* 15A, C203 and *Staph. Oxford* as test organisms a solution reported to contain 3 units per cc produced a zone of inhibition with a radius of 0.8-1.0, 1.1 and 0.9-1.0 cm, respectively.

method of testing consisted in streaking blood agar plates with broth cultures of the organisms and adding Oxford cups filled with the test substance. After 24 hours' incubation at 37° C. the size of the zone of inhibition was measured. It will be seen from the table that all the organisms were sensitive to the antibiotic action of buttercup extract. The thirteen

³ Friedrich Boas and Rudolph Stende, *Biochem. Z.*, 279, 417, 1935.

⁴ Friedrich Keding, *Angew. Botan.*, 21, 1, 1939.

⁵ Gisela Schmidt, *Z. f. Imm.*, 102, 233, 1942.

⁶ E. M. Osborn, *Brit. Jour. Exp. Path.*, 24, 227, 1943.

⁷ We are indebted to Miss Gertrude Herz for technical assistance.

¹ From the department of bacteriology, College of Physicians and Surgeons, Columbia University, New York, N. Y.

² Friedrich Boas, *Ber. deut. botan. Ges.*, 52, 126, 1934.

preparations of juice and two distillates examined gave similar results. However, the measurements of inhibition of growth varied somewhat with different preparations and with the same preparation tested on different days. The figures given in the table represent the minimum and maximum inhibition observed.

H. streptococcus, 15A, was tested for its ability to grow in blood broth in the presence of buttercup juice. Quantities of buttercup representing 0.5 cc, 0.25 cc, 0.12 cc and 0.05 cc of undiluted juice were added to 6 or 7 cc of blood broth which together with controls were seeded with 0.5 cc of a 10^{-8} dilution of a blood broth culture of the streptococcus. The pH of the media was 7.7 before incubation, while after 24 hours' incubation it varied from pH 6.7 to 7.5. After incubation the number of viable organisms were determined by pouring serial dilutions of the cultures. In two experiments the 0.5 cc of buttercup juice inhibited all growth. In a third experiment with another preparation of juice growth was decreased to 10 per cent. of that occurring in the control tubes. Twenty-five hundredths and 0.12 cc of buttercup juice depressed growth also. Five hundredths cc was without effect on the number of organisms growing in 24 hours.

The effect of buttercup juice on the growth of *My. tuberculosis hominis* was tested by adding 5 cc, 2.5 cc or 1.25 cc of undiluted juice to 100 cc quantities of Sauton's media, which was then seeded with H37RV.⁸ Growth failed to occur during a month of observation in any of the nine flasks. The three control flasks showed the usual growth.

The effect of buttercup juice and distillate were tested on the growth of *Candida* (*Monilia*) *albicans*, *Candida* (*Monilia*) *krusei* and *Cryptococcus hominis*.⁹ Pour plates were prepared from glucose/agar melted and cooled to 50° C. to which 0.5 cc, 0.25 cc, 0.12 cc or 0.05 cc of juice was added; all tubes together with untreated controls were seeded with the three microorganisms. The plates were observed at room temperature for two weeks. Five one-hundredths of a cc of juice sufficed to prevent all growth of *Candida krusei* and *Cryptococcus hominis*. Twenty-five hundredths of a cc prevented all growth of *Candida albicans*. All control plates had profuse growth of colonies too numerous to count. In the case of *Candida albicans* the steam distillate obtained from the pressed juice was also tested. One-half cc of this added to the agar resulted in complete inhibition of growth.

Dried plants (*A. pulsatilla*) were obtained from S. B. Penick Co., through the courtesy of Dr. Hock-

⁸ We are indebted to Dr. M. M. Steinbach for preparing these cultures.

⁹ We are indebted to Dr. Rhoda Benham for these cultures.

ing. These were ground, weighed and four times the weight of water added. A preparation of juice and a distillate were obtained as in the case of buttercup. Both were tested against *H. streptococcus* 15A, by the Oxford cup method. The radius of the zone of inhibition was from 1.0 to 1.5 cm. The distillate also was tested against *Candida albicans* by adding 0.5 cc to pour plates of the organism. No growth of the *Candida albicans* occurred. The antibiotic activity of this plant thus compared favorably with that of buttercup.

The toxicity of the buttercup juice for laboratory animals has prevented its use in the therapy of infection. The distillate is less toxic to animals than is the whole juice. Chemical methods are being developed in an attempt to separate the toxic from the antibiotic substance.

CONCLUSION

Pressed juice or steam distillate from the pressed juice of buttercup (*Ranunculaceae* family) is a strong antibiotic with a wide range of activity. It has proven effective *in vitro* in inhibiting the growth of selected Gram-positive and Gram-negative pathogenic cocci and bacilli, *Mycobacterium tuberculosis* and three yeasts, two of which are potential human pathogens.

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DISTRIBUTION OF RADIOACTIVE SULFUR IN THE RAT

IN the course of an investigation of the effects of carbonyl bisulfite on rats bearing tumors, it became desirable to study the distribution, in the animal, of the sulfur-containing moiety of these molecules. The intraperitoneal injection of two carbonyl bisulfites and of sodium sulfate, all containing radioactive sulfur (S^{35}), led to an unexpected accumulation of the active material in the bone marrow of the animal.

Heptylaldehyde bisulfite and cinnamaldehyde bisulfite containing S^{35} were synthesized by passing $S^{35}O_2$ under nitrogen into a slightly alkaline solution containing the aldehydes. Sodium sulfate was synthesized by bubbling $S^{35}O_2$ into an excess of alkaline hydrogen peroxide under a nitrogen atmosphere. All the solutions were adjusted to pH 7.4 and then injected intraperitoneally into rats.

The animals were given water ad lib but were given no food for a period of 14 to 16 hours, at the end of which they were sacrificed. The various tissues were dissected out and aliquots removed for weight and radioactivity measurements. The tissue was decomposed by alkaline fusion,² the melt neutralized, and

¹ We are indebted to Dr. M. L. Crossley, of the American Cyanamid Company, for the radioactive sulfur.

² K. Bailey, *Biochem. Jour.*, 31: 1406-1413, 1937.

the sulfate precipitated as the barium salt. The precipitate was collected following the usual method.³

Measurements of radioactivity were made with a well-shielded G.M. counter tube provided with a thin mica window of the type described by Copp and Greenberg⁴ regulated with a Neber-Haper circuit. Counts were registered by a scale of eight circuit energized by a stable 115 volt A.C. line.

Typical results obtained with the three compounds mentioned above are shown in Table 1. All values are

TABLE 1
DISTRIBUTION OF S³⁵ IN RAT TISSUES

	Heptyl- aldehyde bisulfite	Cinnamal- dehyde bisulfite	Sodium sulfate
Total S ³⁵ injected, counts /min. $\times 10^{-3}$	8.5	110	118
Body weight, gms	115	165	175
Tissue	Counts	/ min.	/ 100 mgm
Bone marrow	218	280	208
Bone matrix	10	14	45
Lymph nodes	4	23	36
Spleen	4	11	18
Thymus	2	7	33
Lymphosarcoma*	4
Liver	3	18	10
Kidney	6	15	39
Brain	3	45	8
Hair	11	130	26

* We wish to thank Dr. J. B. Murphy, of the Rockefeller Institute for Medical Research, for the rats bearing lymphosarcoma tumors used in these experiments.

corrected for radioactive decay and for beta ray absorption in the samples.

It is evident from the table that the highest concentration of radioactive sulfur occurs in the bone marrow irrespective of the sulfur compound used. Though comparison between animals can not be readily made, the data show, nevertheless, that the relative distribution of sulfur in some tissues is somewhat dependent upon the chemical structure of the compound.

Preliminary chemical analysis of the marrow indicates an unexpectedly high sulfur content. The nature of the substances responsible for this high sulfur value are, at present, unknown.⁵

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EXPERIMENTAL VERRUCOUS ENDOCARDITIS¹

On June 11, 1943, we received from Dr. Jacob Werne, of St. Johns Hospital, Long Island City, a

³ We wish to thank Dr. J. R. Rachele, of the Department of Biochemistry of Cornell Medical College, for assistance in setting up a method to collect the radioactive precipitates.

⁴ D. Harold Copp and David M. Greenberg, *Rev. Scientific Instruments*, 14: 205-206, July, 1943.

⁵ We wish to thank G. Horiuchi and H. Levy for technical assistance.

¹ Aided in part by the United Hospital Fund of New York; Grants No. 522 and No. 523 of the Committee on

specimen of fluid removed from the pericardial sac of a woman who had died of active rheumatic carditis. This fluid was passed through a Mandler filter and the filtrate was introduced into culture media (aerobic) with negative result. Some of this filtrate was injected intravenously into rabbits in which there were subsequently found scattered foci of inflammation in the myocardium, in one or more of the valves of the heart and in the pulmonary arterioles. The changes in the valves of the heart could be distinguished from those seen in experimental bacterial endocarditis because of the absence of bacteria, the more diffuse dissemination of the lesions, their curious edematous verrucous character and especially by the remarkable hyperplastic and reparative activity of the endothelium and connective tissue.

During the subsequent eighteen months the pericardial exudate from two more rheumatic patients and the blood from seven others with clinical evidence of rheumatic carditis have been injected into rabbits, guinea pigs and mice, with somewhat inconstant results. It has been possible to recognize damage of endocardial endothelium and connective tissue in most of these animals. The experimental disease has ordinarily not been lethal and the animals have been sacrificed at various intervals after inoculation.

The disease has been propagated in series through fifteen successive animals by injection of blood. In embryonated eggs, inoculation with small amounts of blood from the experimental mammals has given rise to a non-lethal infection which has been propagated in series in the eggs. Allantoic fluid of the eggs has been in turn injected intravenously into rabbits with the production of even more pronounced lesions in the heart, apparently because of a greater concentration of the pathogenic agent in the egg as compared with the mammalian blood.

In addition, to the many rabbits examined in our studies of experimental bacterial endocarditis there have been animals injected with normal rabbit blood, blood of animals dead of various spontaneous disorders, animals injected with normal human blood, normal egg fluids, vaccinia virus, Theiler virus, influenza virus and with the blood of rabbits in which there was evidence of spontaneous disease of the heart. The changes produced in these animals require much further study. At present, however, it appears that they are not identical with the changes seen in the animals of the "rheumatic" series. The distinction between these various disorders of the rabbit's

Therapeutic Research, Council on Pharmacy and Chemistry, American Medical Association; and by the Virus Research Fund of the Lambert Pharmacal Company. This paper was presented in part, together with demonstration of specimens, at the meeting of the New York Pathological Society, New York Academy of Medicine, March 22, 1945.

heart has been very difficult and rather unconvincing when only morphological evidence has been used, as, for example, in the early work of De Vecchi² and the later studies of Andrei and Ravenna.³ In our own studies these difficulties have not been entirely overcome. We have been able to use the more modern method of perfusion fixation to demonstrate more clearly the changes in the valves and in the endothelium of the mural endocardium and aortic intima. The propagation of the supposed pathogenic agent in embryonated eggs and its subsequent passage back to small mammals would appear to have confirmatory

value and would seem to open the field for application of the newer technical procedures of virus study to the agent or agents which may be concerned in the causation of the rheumatic diseases.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

GRAPEVINE INJECTION APPARATUS¹

THE plant-injection procedure for diagnosis of mineral deficiencies causing leaf abnormalities seems preferable in many instances over other types of treatments. Injection into the plant circumvents soil fixation of various ions, requires only a minimum of materials, and also gives assurance that the element in question is at least within the plant. The treatment of single branches provides closely comparable checks and reduces the number of plants necessary for diagnosis. Although foliage sprays have proved useful, there is less assurance of penetration and distribution, particularly in the case of the "nonmobile" elements. For instance, foliage sprays with zinc compounds on zinc-deficient grapevines have given transient and far less marked recovery than that from injections or from daubing the fresh pruning wounds with zinc solutions. The daubing procedure has generally given unsatisfactory results with cane-pruned vines. Injection of 5 ml of 25 per cent. zinc sulfate solution into the trunk has resulted in outstanding improvement in the appearance of zinc-deficient, cane-pruned Thompson Seedless (*Vitis vinifera*) vines. Various injection procedures have been used by Collison *et al.*,² Roach,³ Rumbold⁴ and Wallace,⁵ who refer in turn to many other investigations.

Fig. 1 illustrates the apparatus. The screw is used to force the flat-faced, circular injection point into the wood until the taper at the back end forms a seal

² Bindo De Vecchi, *Arch. de Méd. Expérimentale et d'Anat. Pathol.*, 24: 352-420, May, 1912.

³ Guiseppe Andrei and Paolo Ravenna (translated by Richard Kemel), *Arch. Int. Med.*, 62: 377-387, September, 1938.

¹ Thanks are due to Mr. P. E. Symens for the design and construction of the hook and pump mounting.

² R. C. Collison, J. D. Harlan and M. P. Sweeney, *New York Agr. Exp. Sta. (Geneva) Tech. Bul.*, 192: 1-36, 1932.

³ W. A. Roach, *Ann. Bot.*, n.s., 3: 156-222, 1939.

⁴ Caroline Rumbold, *Am. Jour. Bot.*, 7: 1-18, 1920.

⁵ T. Wallace, *Jour. Pom. and Hort. Sci.*, 13: 54-67, 1935.

with the plant tissue. The solution is then pumped into the tracheal system of the xylem through a hole in the neck of the injection point.

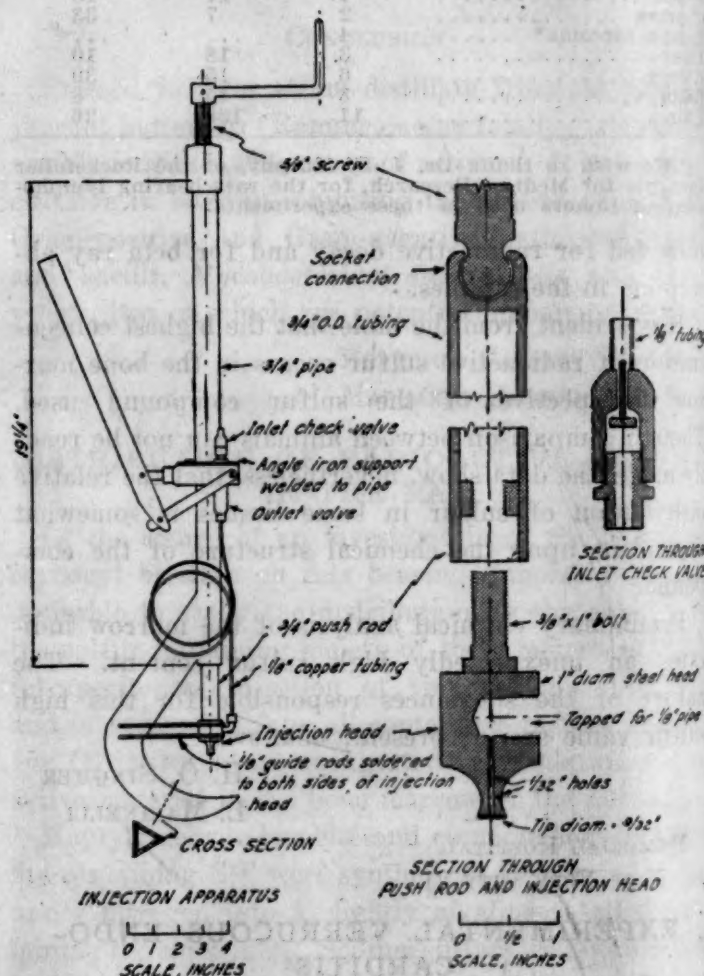


FIG. 1. Grapevine injector.

The injection head was turned from 1-inch soft round steel. The end face of the point is 9/32-inch diameter and is decreased at the neck to 8/32 inch. The neck and sealing face are turned on approximately a 1/4-inch radius. This design allows the solution to flow freely around the neck of the point, and the sheared tracheal ends are not under mechanical pressure which would tend to keep them closed. The

ward sealing flare is short (about $\frac{1}{8}$ inch long) in order to present a large surface area and thus prevent the point from being driven in any further than necessary for sealing. The entire turned section is only $\frac{1}{8}$ inch long, so that chiefly the outer tracheae (which seem to function most effectively in solution induction in the grape) are injected. The head and nut of $\frac{3}{8} \times 1$ -inch S.A.E. bolt were ground down to go inside the $\frac{3}{4}$ -inch O.D. Shelby steel tubing used for the push rod. The nut was welded into the tubing through vee cuts in the tubing. The top of the bolt was sweated (with bronze) onto the center of the back end of the injection head. The hole in the face of the injection tip was covered by sweating (with solder) on a piece of hard brass sheet entirely covering the face. The radial hole in the neck serves for the outlet.

The screw is part of a 4-inch Stearns wood clamp with 6 threads to the inch. The female screw was cut from the clamp, fitted into the end of a $\frac{3}{4}$ -inch pipe coupling and brazed in place. The face of the ball socket on the end of the screw was cut down to $\frac{1}{2}$ -inch diameter and welded to the end of the push rod.

The hook is hollow and, in general, is triangular in cross section. The inner lining is $3/16 \times 1\frac{1}{4}$ -inch strap iron. The vee back is of $\frac{1}{8}$ -inch-thick stock. Patterns for the curved sections of the back were made by cutting cardboard to shape. The rounded hook section does not cause appreciable injury to the grapevine trunk; however, the present design is unsatisfactory for tree injection, as the hook injures the bark tissues too much, probably due to the higher pressure necessary to force the point into the harder wood and to the varietal difference in bark tissues. A 2-inch wide, tree-shaped hook providing two points of contact is suggested, if the device is to be used on trees. The hook is welded onto the $\frac{3}{4}$ -inch pipe, as in the 2-inch section of $1\frac{1}{2} \times 1\frac{1}{4}$ -inch angle iron supporting the pump. Guide rods were bent to shape and soldered to the injection head to prevent the head from turning and twisting off the $\frac{1}{8}$ -inch copper tubing.

The pump is part of a high-pressure, lever-action, grease gun.⁶ The pump cylinder is bolted to the support by a cap screw into the original inlet, and by a male $\frac{1}{8}$ -inch tubing coupling, which served both as a supporting screw and inlet (in place of the original cap screw). A shellacked fiber gasket did not hold the pressure involved, so the pump cylinder was sweated with solder onto the angle iron support.

The valves consist of rubber discs $5/16$ -inch diameter by $\frac{1}{8}$ -inch thick (cut from a rubber stopper) tightly fitting on the shafts against the heads of

$3/32$ -inch diameter nails. The nails (cut to a suitable length) extend into the $\frac{1}{8}$ -inch port holes, which also serve as valve-stem guides. The original attachment tip (coupling) and the check valve provided suitable valve seats and housings. No spring is used on the inlet valve, but a bronze spring replaces the original steel spring of the outlet valve.

A 250-ml dispensing burette mounted on a ring-stand and connected with rubber tubing serves for calibration of the discharge. The bore of approximately 0.4 inch and $\frac{1}{4}$ -inch stroke of the pump described injects about $1\frac{1}{2}$ ml per stroke. If many injections with a single solution are to be made, a hot water bottle serves as a convenient portable reservoir. The pressure used in the injection will need to be regulated by experience, as the vine trunk may be readily split or the seal broken by too high pressures.

A brass (or other corrosion-resistant material) pump and fittings would be desirable; however, the iron may be largely protected by washing out corrosive material and pumping a dilute chromate solution through the instrument each time after use.

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A NEW HERBICIDE, 2,4 DINITRO 6 SECONDARY BUTYL PHENOL

Most herbicides in current use suffer from certain disadvantages. Because of its poisonous properties, sodium arsenite is no longer very popular. Sodium chlorate solution is hazardous because when dried on clothing or vegetation it is highly inflammable. Moreover, supplies of these chemicals have been restricted during the war. Certain petroleum fractions, including diesel fuel, smudge pot oil and stove oil have become rather widely used as herbicides. With the increased demand for oils in the war, however, such use is being restricted and may be entirely cut off.

Recent studies on a series of dinitro compounds of the substituted phenols indicate that they may furnish herbicidal chemicals having properties more desirable than those mentioned above. Such compounds are proving valuable insecticides,^{1,2} and certain ones show promise as herbicides.

The sodium salt of dinitro-*o*-cresol is already in wide use as a selective herbicidal spray on cereals, flax and onions. It was early found that the addition of ammonium sulfate or a strongly acid salt such as sodium bisulfate would greatly increase the toxicity of this herbicide. Such addition was termed "activation." The ammonium salt of dinitro-*o*-cresol proved as toxic as the activated sodium salt. When sodium

¹ J. F. Kagy, *Jour. Econ. Entom.*, 34: 660, 1941.

² G. C. Decker, *Jour. Econ. Entom.*, 36: 658, 1943.

⁶ K-P Manufacturing and Sales Company, 1706 Linden Avenue, Minneapolis, Minnesota.

bisulfate was used as an activator, a white precipitate of the parent dinitro-*o*-cresol was formed which, applied in suspension, proved as toxic as the solution containing the ammonium salt.

From a number of exploratory experiments involving testing of some fifty nitro and chloro compounds of benzene, phenol and various substituted phenols³ the following generalizations seem warranted. Toxicity to plants increases through the series benzenes, phenols, substituted phenols. Dinitro compounds are more toxic than nitro, chloro or nitro-chloro compounds. Ortho substituted salts are more toxic than meta or para. Of the dinitrophenols tested, aliphatic chain substitutions were more toxic than cyclic or aromatic. The 2,4 dinitro compounds of phenol, *o*-methyl, *o*-ethyl, *o*-isopropyl, and *o*-secondary butyl phenol respectively increase in toxicity in the order named. The *o*-amyl substituted compound drops off in toxicity. The ammonium salts of these phenols are soluble enough in water to serve as effective herbicides. The parent dinitro phenols are very slightly soluble in water, but their solubility in oil increases with increasing length of the substituted aliphatic chain. Dinitro phenol is too low in solubility in oil to be of use as a herbicide. Dinitro-*o*-cresol is soluble to about 5 per cent. and has been used as a fortifying agent in diesel oil for weed killing. Because of its high toxicity the secondary butyl compound is more effective, and its relatively greater solubility in oil enhances its value. Table 1 presents toxicity, solubility and composition data on these compounds. Toxicity of the ammonium salts in aqueous solution roughly parallels that of the parent compounds in oil.

In seeking an explanation for the increasing toxicity of the dinitro compounds of substituted phenols it seems that the increasing solubility in oil may offer a clue. As the substituted aliphatic chain becomes longer it influences to an increasing degree the chemical properties of the molecule (*Cf.* Table 1, column 4), the latter becoming more oil-like. In general, substances soluble in lipoids pass readily through cytoplasmic membranes, and penetration of the aliphatic chain into the cytoplasm may well bring about intimate contact with the toxic nitro groups. Toxicity reaches a maximum with the butyl substitution; as longer chains are added, the toxic effect of the nitro groups is apparently "diluted."

The first four compounds listed in Table 1 are solids at ordinary temperatures. The secondary butyl phenol melts at about room temperature, and the amyl compound is liquid at temperatures above -5° C.

³ These chemicals were kindly furnished by the Dow Chemical Company and Standard Agricultural Chemicals, Inc.

Over three million gallons of fuel oil have been used annually in California alone for control of

TABLE 1
SOLUBILITY IN OIL AND RELATIVE TOXICITY* OF DINITRO COMPOUNDS

Chemical	Solubility† in kerosene, at 20° C	Toxicity	Chain Wt. x 100 Total MW
	per cent. (approx.)		
Dinitro phenol	0.14	38	0.0
Dinitro- <i>o</i> -cresol	0.58	64	7.5
Dinitro- <i>o</i> -ethyl phenol	2.39	75	13.6
Dinitro- <i>o</i> -isopropyl phenol	3.43	90	18.9
Dinitro- <i>o</i> -secondary butyl phenol	miscible	100	23.6
Dinitro- <i>o</i> -secondary amylphenol	miscible	90	27.8

* Toxicity in arbitrary units has been calculated to a basis of 100 per cent. for dinitro-*o*-secondary butyl phenol, the most toxic of the six compounds. This would require concentrations of approximately 0.5 per cent. on grasses and 0.125 per cent. on broad-leaved weeds if applied in non-toxic oil or oil emulsion under the conditions of these experiments.

† Solubility is higher in more polar solvents. The aromatic and olefin contents of petroleum fractions largely determine their solvent power for the nitrophenols.

weeds. By mixing dinitro-*o*-secondary butyl phenol in such an oil at a concentration around 5 to 10 per cent. and adding emulsifiers so that the mixture will form a fairly stable ready-mix type of emulsion, a general contact herbicide can be made that would require only 3 to 6 per cent. oil. Such a mixture would effect a saving of over 90 per cent. of the oil that has been used for weed control; it will kill certain oil-tolerant weeds that are not ordinarily controlled; and, if made up in sufficient quantities, it should not exceed oils in cost. Mixed, as suggested above, it makes a concentrated stock solution that may be emulsified with water in the field. This saves hauling costs that are high with oils. The materials are not dangerously inflammable; the poison hazard is much less than with arsenic, livestock not being attracted by the taste as they are by sodium arsenite.

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